

Scaling Smart Matching Platforms: Evidence from Multi-Country Randomized Trials in School Choice

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Abstract

Governments in developing countries are moving school admissions onto centralized platforms. In Chile, warning families on such a platform that their child might go unplaced led them to add schools and reduced non-placement. We study whether this result replicates in the first centralized school-choice systems of Peru and Ecuador, where ministries sent families report cards with their placement risk by WhatsApp and email days before the deadline, after they had applied. In Peru, warned families revised their applications and families without the warning barely did: in 2022, 9.3 percent of warned applicants added a school, against 1.7 percent of those below the warning threshold, and a discontinuity at the threshold points the same way as in Chile. In 2021, warned families were 9.5 percentage points less often unplaced in the first round than comparable families who were not contacted, and simulating the assignment shows 110 more children placed overall, net of displacement. Surveys built on the Chilean questionnaire find the same frictions: families know a fifth to a third of nearby schools and overestimate their admission chances by 20 to 37 percentage points, supporting the generality of the mechanism. Randomized lists of suggested schools, an exploration set out in our pre-analysis plan, redirected Peruvian applications toward the listed schools. In Ecuador, where the warning appeared only on the report card and reached fewer families, the response was weaker, and the lists had no detectable effect.

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1 Introduction

In markets that allocate scarce places through a ranked application, the price an applicant faces is the probability of being accepted, and nobody posts it. Families applying for school seats, like job seekers or households on a housing waitlist, must decide how much to search without observing their chances; this paper shows what happens when a government tells them. Governments across the developing world are moving school admissions onto centralized platforms, on which families submit ranked applications and an algorithm assigns seats ([World Bank, 2024](#); [Neilson, 2024](#)). Peru and Ecuador introduced centralized school choice for the first time for 2021 admissions, in the region of Tacna and in the Manta area, for children entering preschool or, in Peru, first grade. The procedures these platforms replaced were costly and open to manipulation. In Peru there were many reported cases of parents paying fees or bribes to secure seats in oversubscribed schools, and parents often queued for days to apply ([Elacqua et al., 2022](#)). In Ecuador, students had been assigned by the home location reported through an electricity bill, which gave families an incentive to obtain bills from areas near the most selective schools ([Elacqua et al., 2024](#)).

Centralization is meant to remove these costs, but it creates a new task: families must decide which schools to rank, often knowing few of their options and misjudging their chances. As [Kapor et al. \(2020\)](#) show, heterogeneous beliefs about admissions chances distort application behavior and can worsen outcomes for less-informed families.

One policy response is to adopt strategy-proof mechanisms that make truthful reporting optimal regardless of sophistication ([Abdulkadiroglu and Sönmez, 2003](#); [Abdulkadiroğlu et al., 2017](#)). Part of the appeal of that approach is that knowledge of admissions chances, which is costly to acquire and unequally distributed, should not be required for optimal play. That argument, however, presumes applicants already know which options are available to them. Strategy-proofness disciplines how applicants rank the schools they have found, but says nothing about how many they look for, and when search is costly, beliefs about admissions chances re-enter through the decision of when to stop searching ([Ajayi and Sidibe, 2024](#); [Hakimov et al., 2023](#)).

A complementary approach is to embed “smart” features into application platforms that provide real-time feedback and guide applicants toward safer or higher-value options. Evidence from Chile’s national school assignment system shows that “Smart Matching Platforms” offering placement risk alerts help families overcome optimism bias and avoid applying to too few schools (Arteaga et al., 2022). Follow-up evidence associates these short-run improvements with sustained gains in academic achievement up to five years after initial placement, at low cost per student (Arteaga et al., 2026). Similar tools have since been deployed in Colombia, Brazil, and Peru, where digital assistants and chatbots have helped students gain admission to higher-quality institutions (World Bank, 2024).

Deployment at that scale raises a separate question. Behavioral interventions run at scale by government units show smaller effects than the academic trials that inspired them, a gap that DellaVigna and Linos (2022) attribute to publication bias and low power in the academic literature rather than to who runs the program; the scaling literature documents other reasons an effect can shrink once an implementing agency takes a program over (Al-Ubaydli et al., 2020; List, 2022). Arteaga et al. (2022) argued that smart matching platforms are less exposed to those risks, because governments already operate them, and cited as examples the implementations then underway in Brazil, Peru, and Ecuador. Peru and Ecuador are two of those countries, and their implementations differ from the Chilean evidence in four ways. First, the ministries delivered the information outside the application platform: families at risk were sent a personalized report card, and in most arms a warning, by WhatsApp (and, in 2021, by email) four to five days before the deadline, rather than shown a warning on the platform itself. Second, the systems were new: both pilots introduced centralized school choice, and we study Ecuador’s first year and Peru’s first two. Third, as in Chile, the warning followed a fixed risk rule set by the ministry, and we used randomization to explore a further question, whether recommending specific schools near home adds to the warning. Fourth, the implementation shows where delivery depends on the state. We ask whether the Chilean results replicate across ministries and markets that differ in the frictions they face, the alternatives they offer, and the machinery available to deliver the information.

Our evidence comes from interventions run with the Ministries of Education

of Peru and Ecuador, covering 2,021 applicants in Ecuador in 2021 and 1,708 and 1,140 applicants in Peru’s 2021 and 2022 cohorts, within which the content of the report card was randomized.¹ Families received a personalized report card on their application, and families at risk were warned that their child might go unplaced; randomly assigned arms added a list of suggested schools near home. In Peru the warning also came as a separate WhatsApp message. In Ecuador no separate warning was sent, and the report card carried the warning only for the 994 applicants (49 percent) whose simulated non-placement risk was at least 1 percent; the other 1,027 had simulated risk below 1 percent, most of them exactly zero. We use scale in a market sense: the experimental samples cover about half of all applicants in Ecuador and about a quarter in each Peruvian cohort, not a small subset, although we do not observe a national rollout. This design allows us to examine both (i) the transportability of behavioral responses and (ii) which implementation and market conditions differed where effects did and did not appear.

Using linked survey and administrative data, we document three motivating facts. First, families are poorly informed about their options: respondents knew, at least by name, between a fifth and a third of the nearby schools their report card would list (36 percent in Ecuador and Peru 2021, 21 percent in Peru 2022). Second, applicants at risk are overly optimistic about their chances, overestimating admission probabilities by 20 to 37 percentage points on average depending on context. Third, families rate being left unplaced as far worse than any placement. In practice, most applicants unplaced through the platform are later observed enrolling, usually at a school on the platform through a later round or a direct vacancy, so the cost of non-placement is mainly delay, uncertainty and a seat among those left over; 22 percent of Peruvian applicants unplaced in 2021 are not observed enrolling, though enrollment records are incomplete (Section 5). Our surveys were built on the Chilean questionnaire, and the Chilean survey shows

¹The pre-analysis plan is dated February 2021 and the trials were entered in the AEA RCT Registry (AEARCTR-0007122) on March 1, 2021. Only the Peru 2022 wave was fielded entirely after registration: the Peru 2021 wave was fielded in January 2021, and the Ecuador 2021 report cards were sent between February 25 and March 3, 2021, most of them on February 26. The design of the Peru 2022 wave (its 0.30 risk threshold, three-school arm and two-district sample) was decided after registration, so we treat that wave as exploratory (Section 4).

the same pattern: large optimism about admission chances, and the same two leading reasons for stopping search. That these frictions recur in three countries with different school systems and markets adds plausibility to the generality of the mechanisms the warning targets.

Our main result is that warning families works in Peru as it did in Chile. Families had already submitted their applications, and most would not have changed them. In 2022, when every family could revise, only 1.7 percent of applicants below the warning threshold, who received a plain report card, added a school; above it, 9.3 percent did (Table 4). At the threshold itself, the design of the Chilean study, the share of families adding a school jumps by 6 to 8 percentage points among families whose report card carried the warning, and the number of schools added by about a third of a school in the global fit (0.34, s.e. 0.16); these estimates are imprecise in narrow windows. Families who received the warning only as a WhatsApp message, with a report card that did not carry it, show no jump, although the difference between the arms is imprecise (p between 0.10 and 0.40 across bandwidths) and the randomized contrasts above the threshold show no difference in the share adding any school. In 2021, when families who were not contacted could not revise at all, 34 percent of warned families who opened the report card added a school. The revisions improved placement. Warned families were 9.5 percentage points (s.e. 2.9) less often unplaced in the first round than comparable at-risk families who were never contacted, after adjusting for baseline risk, and the reduction grows with risk, to 16.5 percentage points (s.e. 4.6) in the top quartile. Because seats are fixed, a gain for one family can come at another’s expense, so we also simulate the first round with the warned families’ lists before and after the report card: the expected number of unplaced children falls by 110, or 6.6 percent, net of the 30 placements other applicants lose, and in 2021, when every report card carried the warning, families who received it without a list gained nearly as much as those who also received one. These comparisons are not randomized: the 2021 comparison group could not revise, and contacted families started at higher risk, so without the adjustment the gap is 3.2 percentage points (s.e. 3.2).

The randomized arms explore what recommendations add to the warning, as set out in our pre-analysis plan, motivated by the survey finding that families

know few of the schools near them. In Peru’s first cohort, a list of ten suggested schools raised the share of applicants who added a listed school by 6.3 percentage points among all assigned applicants (s.e. 2.4), from a base of 18 percent, and by 12.6 percentage points among families who opened the report card (s.e. 3.2). The list changed which schools families added more than how many, and effects were smaller in the second cohort (2.4 percentage points, s.e. 0.9, for a three-school list). Adding a listed school is the first secondary outcome of the plan; neither primary outcome, the number of schools listed or placement through the platform, shows an intention-to-treat effect of the list significant even at the 10 percent level in any wave (Table 7). No wave was fielded under a plan registered beforehand that also described its design: the plan was registered after the first Peruvian wave was fielded and while the Ecuador wave was under way, and the design of the second Peruvian wave was decided after registration. We therefore use the plan’s outcome list as a discipline, report both primary outcomes for every wave, and treat the evidence on recommendations as suggestive.

In Ecuador the response was weaker. The warning appeared only on the report card, with no separate WhatsApp message, and only for the 49 percent of applicants whose simulated risk was at least 1 percent, and fewer families opened the card (43 percent, against 63 percent in Peru’s first cohort). Among openers whose card carried the warning, 15 percent added a school, against 34 percent of warned openers in Peru’s first cohort. The recommendation arms had no detectable effect, even among openers: the ten-school list changed the share adding a listed school by 0.5 percentage points (s.e. 2.4), 12.1 percentage points (s.e. 4.0) less than in Peru’s first cohort, and the shortfall persists among openers whose card carried the warning. Low reach therefore does not explain that null, which coincides with families who barely searched: without a list, 9.7 percent of openers added any school, and most of those who did chose one the list would have named. Ecuador also differed in ways the design cannot separate. Its families, previously assigned a school by address, were choosing for the first time; they could revise their applications at any time; and on phones the card may have placed the list behind an expand button. We use the cross-country comparison to discuss, not to establish, the conditions under which the intervention transports.

The implementation also shows how the state shaped what could be delivered

and learned. In Peru, officials treated a warning that a child might go unplaced as politically costly. The ministry would not randomize the warning and required it to follow a transparent rule it could defend publicly: in 2021 it permitted outreach to applicants with positive risk in the covered grades, and in 2022 it restricted the warning to applicants above a 0.30 risk threshold, which it accepted in part because Chile already used it. What could be randomized was the content of the report card.

Our first contribution is a replication: proactive information on admission chances changed applications and placement in a new school-choice system run by another government, as it did in Chile ([Arteaga et al., 2022](#)). The findings also contribute to the literature on information provision in education markets ([Hastings and Weinstein, 2008](#); [Andrabi et al., 2017](#); [Allende et al., 2019](#); [Borger et al., 2024](#)). The closest design to our recommendation arms is the randomized distribution of curated lists of nearby high schools in New York City, which steered students away from schools with low graduation rates ([Corcoran et al., 2018](#); [Cohodes et al., 2025](#)). In Chile’s centralized college admissions, personalized information on admission chances, developed with the Ministry of Education, improved admission outcomes in a field experiment and again after the government extended it to all applicants ([Larroucau et al., 2025](#)); our results show a similar transport for school choice in Peru, and its limits in Ecuador. The findings bear on empirical market design work on how beliefs and search frictions shape application behavior in low-information environments ([Kapor et al., 2020](#); [Ajayi and Sidibe, 2024](#); [Arteaga, 2024](#)).² Finally, they add to the evidence on the external validity of policy experiments. Interventions that are effective in one setting often lose much of their effect at scale, in new populations, or under different implementation conditions ([Allcott, 2015](#); [Vivalt, 2020](#); [Banerjee et al., 2017](#); [Muralidharan and Niehaus, 2017](#); [Al-Ubaydli et al., 2020](#); [List, 2022, 2026](#)), and what policymakers need to transport is a benefit-cost profile, not only a treatment effect ([Davis et al., 2017](#)). We report one design, fielded by two governments in the same year, that worked in Peru and only weakly in Ecuador.

The remainder of the paper is organized as follows. Section 2 presents the conceptual framework and the predictions it generates. Section 3 describes the

²For a survey of market design, see [Agarwal and Budish \(2021\)](#).

settings in Peru and Ecuador, Section 4 the intervention, and Section 5 the survey evidence on what families know and believe. Section 6 reports the results on choice behavior and placement. Section 7 draws on the comparison across countries to discuss the conditions under which the intervention scales, and Section 8 concludes.

2 Conceptual Framework: Search and Biased Beliefs in Application Markets

2.1 A general class of markets, and what distinguishes it

Costly search over a limited consideration set, combined with biased beliefs, arises in job search, provider choice, and consumer search. This generality, however, makes the framework too coarse to predict when an information intervention will work. We therefore focus on a narrower setting in which applicants submit a single ranked list and a mechanism allocates scarce places. In these markets, each option is characterized by a probability of admission rather than a price. Our framework builds on sequential search in the tradition of [McCall \(1970\)](#), while incorporating the departure that [Arteaga et al. \(2022\)](#) identify: families add discovered schools to a portfolio that the mechanism evaluates all at once. That departure changes the search problem in three ways.

First, the relevant price is not posted. In a posted-price market, a searcher observes an option’s terms before deciding whether to continue searching. Here, the comparable object, the probability of admission, is not directly observed. Families must infer it from other applicants’ choices, school capacities, and the allocation mechanism; Section 5 documents large, systematic errors in those inferences. Second, these belief errors compound along a ranked list. Because the mechanism evaluates the list in order, a modest error about each school can propagate through the entire list rather than remain local to one option, as a posted-price error would (Section 2.2). Third, search ends before allocation feedback arrives. A family submits one list and cannot observe a rejection and then continue searching within the same cycle. It must therefore assess its admission

chances before search concludes, when those chances are least knowable.³

Markets sharing these features include centralized school choice and clearing-houses such as the medical residency match (Rees-Jones and Skowronek, 2018); rationed allocations that offer units over time, such as public housing, share the problem of choosing without observing one’s chances, and choice errors matter there too (Naik and Thakral, 2026), although applicants do not submit a single list. In each, applicants must decide how much to search without observing their chances of receiving a given option. The price system is therefore incomplete. In school choice, a smart matching platform can fill this information gap by displaying placement risk while the list remains editable. We use the framework to explain how biased beliefs distort list length and to generate testable predictions. The portfolio and optimism primitives are drawn from Arteaga et al. (2022) and Agte et al. (2024), who develop and structurally estimate the full search-and-application model.

2.2 The portfolio problem and the compounding of belief errors

Formally, suppose that a family initially considers a set $C_0 = \{1, 2, \dots, K\}$ of schools, ordered so that $u_1 > u_2 > \dots > u_K > 0$, where u_j is the expected utility of school j . Let $p_j \in [0, 1]$ denote the (subjective) probability of being admitted to j when it is ranked. Under a deferred acceptance assignment, the expected value of the portfolio $(1, \dots, K)$ can be written as

$$V_0 = \sum_{k=1}^K \left(\prod_{j < k} (1 - p_j) \right) p_k u_k, \quad (1)$$

where the product term is the probability that the family is rejected by all schools above k but admitted to k . This expression highlights the *portfolio* nature of the problem. The value of a list depends not only on the schools it contains, but also on their order. The product operator also makes belief errors compound across ranks.

³Where a system runs later rounds, as Peru’s does, this holds within a round, and the later round becomes one component of the fallback u_0 introduced below.

Following [Agte et al. \(2024\)](#), we summarize optimism about admission chances with a parameter $a \in [0, 1)$ and write risks as

$$R_j \equiv 1 - p_j, \quad R_j = (1 - a)R_j^*,$$

where R_j^* is the true non-placement probability at school j . Higher a means families understate risk and overstate admission probabilities. Under this parametrization, the perceived probability of reaching rank r in the list is

$$\prod_{j < r} R_j = (1 - a)^{r-1} \prod_{j < r} R_j^*. \quad (2)$$

The predictions below follow from equation (2). The gain from adding a school at position r is proportional to the probability of ever reaching that position, which optimism scales by $(1 - a)^{r-1}$, a factor that decays geometrically in list position. A family only modestly optimistic about each individual school can therefore be badly wrong about the value of a fourth or fifth option, and optimistic families see little reason to search at all.

Two testable implications follow. First, the wedge between the perceived and true probability of reaching position r ,

$$[1 - (1 - a)^{r-1}] \prod_{j < r} R_j^*,$$

is increasing in the true risks R_j^* : families genuinely exposed to non-placement have the most understated returns to search, so a given belief correction should move them most. Second, because the distortion operates on the perceived value of searching, correcting it should induce portfolio expansion rather than mere reshuffling.

Families also face a (possibly heterogeneous) search cost C to discover a new option s and learn its characteristics. Let $E[\Delta V \mid C_0, a]$ denote the ex ante gain from one additional search, given the current consideration set C_0 and beliefs summarized by a . A family searches (and potentially adds a school) only if

$$E[\Delta V \mid C_0, a] \geq C. \quad (3)$$

Biased beliefs about admission chances (high a) or about the quality of new schools (low perceived u_s) both reduce $E[\Delta V \mid C_0, a]$, causing families to under-search and maintain smaller, more selective portfolios. Over time this generates an endogenous information friction: families start with a limited consideration set C_0 and choose not to expand it, so knowledge of the available options stays imperfect and lists stay short.

Equation (1) implicitly values non-placement at zero, but in practice a family that is not placed through the platform is not left with nothing: it may enroll off-platform or be placed in a later round. Let $u_0 \geq 0$ denote the value of this fallback, the family’s outside option, so that the portfolio is worth

$$V_0 = \sum_{k=1}^K \left(\prod_{j < k} R_j \right) p_k u_k + \left(\prod_{j=1}^K R_j \right) u_0,$$

and the gain from discovering and adding a school s at the end of the list becomes

$$\Delta V = \left(\prod_{j \leq K} R_j \right) p_s (u_s - u_0). \quad (4)$$

The return to search is therefore the surplus over the outside option rather than the gross value of the school. Perceived ΔV depends multiplicatively on a and u_0 , creating an identification problem. An optimistic family and a family with a favorable fallback can submit equally short lists, so portfolios alone cannot distinguish them. Two sources of variation help separate the two. First, the warning shifts beliefs while leaving $(u_s - u_0)$ unchanged. It should therefore change list length only when short lists are driven by mistaken beliefs. Second, Section 5 asks families directly. Equation (4) also sharpens the market-density condition below: what governs the returns to search is not the number of nearby alternatives but the number that improve on the fallback.

2.3 Testable predictions

Equations (2) and (3) identify two levers that act on different arguments of the search rule. A warning reporting personalized non-placement risk operates on beliefs by lowering a and raising $E[\Delta V]$, whereas a suggestion list operates on

search costs by lowering C and enlarging C_0 . This distinction yields five predictions, denoted P1 through P5.

The first two concern the warning. By lowering optimism, a personalized non-placement warning raises the perceived return to search in (3), so recipients should add schools to their portfolios relative to otherwise identical applicants (**P1**). Its effect should be larger for applicants with higher baseline risk. The gap between perceived and true returns is proportional to $\prod_{j < r} R_j^*$, so the same belief correction yields a greater return to search when true non-placement risk is higher. The warning’s effects on portfolios and realized placement risk should therefore both increase with baseline risk (**P2**).

The next two concern the suggestion list. Lowering C and enlarging C_0 makes particular alternatives cheaper to evaluate. Conditional on a family having already been warned, so that beliefs have been corrected and the decision to search has been made, the list should shift applications toward the schools it names without necessarily lengthening the list. It changes which option is cheapest to add rather than whether adding an option is worthwhile (**P3**). If its central contribution is to lower the cost of identifying a relevant alternative rather than to widen a family’s choice set, the number of suggestions should not be the binding margin. Effects should be similar whether families see three suggestions or ten (**P4**).

Finally, P5 concerns conditional transportability. The same primitives that generate P1 through P4 also determine whether the intervention travels. Effects should attenuate where baseline optimism a is smaller, where awareness C_0 is already broad, where the local market offers few feasible alternatives, and where information fails to reach families in time to act (**P5**).

These predictions organize the empirical analysis that follows. Section 5 first documents limited awareness (small C_0), optimism (high a), and stated search costs (C). Section 6 then tests the warning predictions, P1 and P2, by comparing families who received the warning with families who did not, at the 2022 risk threshold and across contacted and never-contacted families in 2021, and by tracing the effect on placement; it tests the suggestion-list predictions, P3 and P4, with the randomized arms. Section 7 asks whether the cross-site pattern is consistent with P5; with three implementations it cannot test it.

2.4 When the intervention transports

The framework also identifies the conditions that determine whether treatment effects travel across settings. The inputs that govern a family’s response to the intervention vary across markets, and differences in those inputs can therefore produce different effects. Four margins matter. The first three follow directly from the model. The fourth emerged in implementation and has received comparatively little attention in the scaling literature.

First, treatment effects depend on the size of the frictions the intervention is designed to correct. Where families are less optimistic about admission chances (low a), the warning produces smaller belief updates and portfolio adjustments; where they already know most local options (large C_0), the suggestion list adds little. A response also requires families to know how to act on updated information. In markets where school choice is new, limited experience can impede strategic list building. Misunderstanding of the mechanism creates a related barrier. Families who believe that adding schools harms their higher-ranked chances perceive expansion as costly, weakening any information response.

Second, the returns to search depend on the richness of the local school market. Lower school density reduces the set of feasible alternatives, limiting how much families can improve their portfolios even with corrected beliefs and lower search costs. Market congestion matters too. When fewer families face meaningful placement risk because seats are abundant relative to demand, the policy-relevant target population shrinks, and the intervention’s aggregate impact falls even if its per-family effect does not.

Third, none of this can operate unless the information reaches families. Delivery reach, the share of targeted families who receive and engage with the information, bounds the effect of any rollout. Reach alone does not ensure a response: the information must also be noticed, so a warning embedded in a dense report card may be less effective than a separate alert on a familiar channel, and it must arrive early enough for families to search and modify their lists.

Fourth, what an agency is willing to deliver depends on how costly it perceives the message to be, not only on its technology. Warning that a child is likely to go unplaced is politically uncomfortable at scale, and that cost rises with the number of recipients in a way delivery costs do not. Sections 6 and 7 return to this point,

which shaped our design.

Equation (4) yields a practical diagnostic for when a warning changes behavior. Families must both misperceive their admission chances and perceive alternatives that improve on the fallback. A warning that confirms existing beliefs moves nobody, and a family that sees no alternative better than u_0 is informed rather than helped. The second condition is easy to miss because it is not visible in risk data and emerges only when families report how they view their options. Both conditions can be measured with short surveys before rollout and combined with market density from administrative data, delivery infrastructure from pilot outreach, and the agency’s tolerance for communicating risk. Section 5 documents the two conditions, and Section 7 evaluates all of these inputs across settings.

3 Setting: Cities in Two Countries

We study information provision in Manta, Ecuador and Tacna, Peru, both implementing centralized school choice for the first time. This offered an opportunity to test a common design in different contexts. In both pilots parents applied through an online platform, students were assigned by a deferred acceptance (DA) mechanism, and we applied similar information treatments.

We were granted access to application and enrollment records and complemented these data with a survey of participating parents. This access allows us to observe the universe of applicants and the history of their applications. In addition, we observe information on options available through the platform and on off-platform alternatives, together with families’ final enrollment decisions for the 2021 academic year.

The IADB supported the pilots in both countries, but their origins were quite different. In Ecuador, the government wanted to introduce parental choice to improve efficiency and equity in school access. The country had previously used a centralized system that assigned students to schools based on the applicant’s location, which parents reported through their electricity account code (CUEN). This process was costly and time-consuming as it required considerable effort to ensure that the assignment results were consistent with existing transportation options, and that routes to school were not blocked by hills, rivers, or other geographic

barriers. The reporting system also created incentives to obtain electricity bills from areas near the most selective schools. These problems made assignments less transparent and less predictable.⁴

In Peru, the government’s objective in introducing centralized assignments was instead to improve the transparency of the school system. There were many reported cases of parents paying fees or bribes to ensure their children received admission to certain oversubscribed schools. Parents also often waited in long lines for days to apply for a vacancy in a selective school (Elacqua et al., 2022).

3.1 Ecuador

To replace the CUEN-based assignment described above, the government partnered with IADB and ConsiliumBots to introduce parental choice through a centralized process, piloting it in Manta, which the ministry selected after a search for a small but representative city in which to scale the policy: the criteria were the number of students in the urban area, school coverage, the mix of public and private schools, and city size, and Manta was chosen for its similarity to the alternatives (Elacqua et al., 2024). With 221,000 inhabitants and 153 schools, Manta is a tenth the size of Guayaquil, but its public-school share of entry-level enrollment, 67 percent, and its share of three- to five-year-olds are close to the larger city’s, and local authorities supported the policy change.⁵

For the 2021 admission, the Ministry of Education (MoE) collected applications to national public preschools (ages 3-5) in Manta through the new centralized online platform. Based on families’ submitted rank-order lists (ROL), students were assigned to one of their options using the deferred acceptance algorithm (also employed in Peru). The system covered three districts (namely, Manta, Jaramijó, and Montecristi), an area representing 2.5% of the national school enrollment.

The application process consisted of a single round. The online platform opened on February 11, 2021, and families had about three weeks to complete their applications, with no limits on the number of schools they could include on

⁴See Elacqua et al. (2024) for details on the distance-centric algorithm used in Ecuador and qualitative evidence for distortions in the electricity bill registration process.

⁵See Elacqua et al. (2022) for further information on the Ministry of Education’s rationale for choosing the region of Manta.

their lists. They could also modify their rank-order list multiple times.

Applicants could also apply to three types of institutions providing preschool education that were not listed on the online platform. These were municipal public schools, subsidized private schools, and private schools, with different ownership and management characteristics. Schools outside the national public network represented 45% of the possible options, and defined their own application processes.

The universe of applicants assessed here consists of around 4,000 children aged 3-5, with a balanced number of boys and girls. Column 1 of Table 1 shows that 3,984 applicants submitted a rank-order list for the 2021 admission process. The average length of the final portfolios was 1.9 schools, and around 66% of the families applied to schools in Manta, the largest district. 43% of the applicants requested a place in “pre-pre-kindergarten” (three-year-olds), and 43% in “pre-kindergarten” (four-year-olds). If we count the schooling options within 2 kilometers of each household, we observe that, on average, there are slightly more off-platform options (municipal public schools, subsidized private schools, and private schools) than on-platform options (national public ones).

Table 1: Descriptive Statistics for Applicants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ecuador 2021			Peru 2021			Peru 2022		
	All	RCT	Pre	All	RCT	Pre	All	RCT	Pre
		Sample	Placement Survey Sample		Sample	Placement Survey Sample		Sample	Placement Survey Sample
Female	0.49	0.50	0.50	0.49	0.48	0.50	0.50	0.54	0.50
From largest district	0.66	0.71	0.70	0.43	0.58	0.49	0.40	0.65	0.44
Length pre-treatment portfolio	1.83	1.79	1.96	3.14	2.97	3.24	3.16	2.85	3.30
Length final portfolio	1.90	1.91	2.05	3.34	3.81	3.50	3.22	3.09	3.39
In-platform opts in 2 km radius	11.32	13.10	12.46	16.33	18.55	17.01	16.66	19.90	17.46
Off-platform opts in 2 km radius	12.11	13.94	13.37	19.28	27.98	21.05	20.30	29.58	22.26
<i>Grade</i>									
PPK (3 yrs old)	0.43	0.43	0.43	0.35	0.37	0.38	0.35	0.28	0.37
PK (4 yrs old)	0.43	0.45	0.44	0.10	0.00	0.09	0.12	0.11	0.11
K (5 yrs old)	0.14	0.12	0.13	0.06	0.00	0.06	0.04	0.03	0.04
1st (6 yrs old)	0.00	0.00	0.00	0.49	0.63	0.48	0.49	0.58	0.48
N	3,984	2,021	1,872	6,876	1,708	1,721	4,856	1,140	1,501

Notes. All statistics are means in the population defined by the column header. Largest district is Manta for Ecuador and Tacna for Peru.

3.2 Peru

In Peru, families have historically applied directly to each school in person. In 2021 and 2022, in an effort to improve transparency and efficiency in student assignment, the government worked with IADB and ConsiliumBots to introduce a centralized student assignment pilot in the region of Tacna, one of the objectives being to eventually scale this reform up to more regions.⁶ The government chose Tacna because it was a small region with a high concentration of schooling options. Additionally, the local government was a strong proponent of the reform.⁷ For the 2021 and 2022 admission processes, all applications to public schools in Tacna were submitted on the new centralized online platform using a rank-order list (ROL), following which families potentially received a placement offer from the Ministry of Education (MoE).

The system covered 10 districts,⁸ accounting for close to 1% of national school enrollment, and governed placement in preschool through grade 1 (ages 3–6). As in Ecuador, students were assigned to one of their ranked options through the deferred acceptance algorithm. The process had three rounds, but we focus on the first, when our intervention operated. The platform opened in early December in both years and imposed no limit on list length. In 2021, families had seven weeks to apply and could modify their lists only with system-authorized access. In 2022, the first-round window closed on December 26 and families could revise their lists without limit.

The 2022 intervention covered only the region’s two largest districts. The report cards required congestion and popularity measures that were available only there, so the experimental sample was limited to those districts. All 1,140 applicants in the Peru 2022 experimental sample were in them, with 739 in the largest district and 401 in the second. The 2021 sample spans all ten districts. Comparisons between the two Peruvian waves therefore contrast a region-wide cohort with a two-district one.

⁶This process was delayed due to the COVID-19 pandemic. In 2023, the government began introducing the reform in two additional regions: Arequipa and Madre de Dios.

⁷See [Elacqua et al. \(2022\)](#) for more details on the government’s rationale for choosing the region of Tacna.

⁸Namely, Alto de la Alianza, Calana, Ciudad Nueva, Gregorio Albarracín, Inclán, La Yarada los Palos, Pachía, Pocollay, Sama, and Tacna.

Applicants could also apply to private schools that were not listed on the online platform. In Peru, private schools compete in the provision of PK-11 education,⁹ and interested families can apply directly to each school, following a decentralized process that is not coordinated with the public school choice process. In Tacna, the largest district participating in the pilot, private schools represent 40% of the available options. These schools charge tuition fees, do not receive funding from the government, and since a reform in 2012, are not allowed to engage in active selection or discrimination of students.¹⁰

The universe of assessed applicants in Peru for both years includes around 11,700 families with children aged 3-6, with a balanced gender ratio. Columns 4 and 7 of Table 1 show that 6,876 applicants submitted an ROL in the 2021 intake, while 4,856 applicants did so in 2022. The average length of the ROLs submitted was 3.3 schools, and around 40% of the families applied to schools in the largest district. 49% of applications were for a grade 1 seat and 35% for “pre-pre-kindergarten” (three-year olds). If we count the schooling options within 2 kilometers of each household, we observe that, on average, there are more private options (off-platform), than public ones (on-platform).

3.3 Differences Between Manta and Tacna

The pilot regions differ in three ways that matter for interpreting behavioral responses. First, Peruvian families had experience applying directly to schools under the old decentralized system, whereas Ecuadorian families had never chosen, having been assigned by location. That difference alone could shape list length, and so could the markets themselves: initial (pre-intervention) applications listed 3.15 schools on average in Peru and only 1.83 in Ecuador, making Peruvian lists 72% longer (Table 1).

The averages understate the gap. The single-school application is the norm in Ecuador. Sixty-one percent of families rank exactly one school on their initial application, compared with 4.6% in Peru 2022 and almost none in Peru 2021,

⁹Ages 3-17; see Allende (2019) for an in-depth discussion of the Peruvian school system.

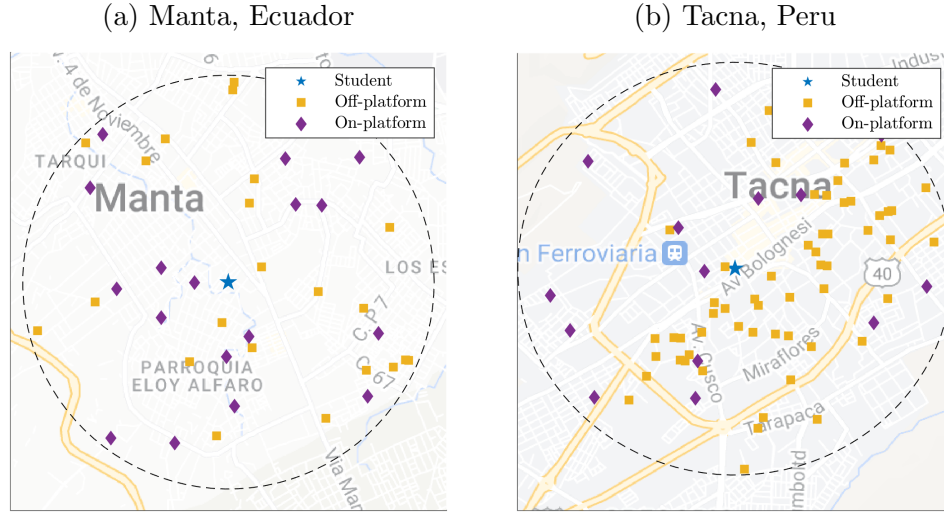
¹⁰For details, refer to *Directiva N 014-2012-MINEDU/VMGP “Normas y Orientaciones para el Desarrollo de Año Escolar 2013 en la Educación Básica.”* However, recent evidence shows that private schools continue to use different instruments to screen students and families, including cognitive tests and interviews (Balarin et al., 2019).

and the share barely moves over the application window.¹¹ Market thinness alone does not account for it: a family listing one school still has ten further on-platform options within 2 km, although that set is thin relative to Peru’s sixteen. Both margins are in play: how families approach a platform they have never used and how many alternatives the market gives them.

Second, the density of schools is lower in the Ecuadorian context. Table 1 shows that an average applicant from Peru had 19 off-platform options and 16 on-platform ones. In Ecuador, in contrast, families had an average of 12 off-platform and 11 on-platform options. Figure 1 illustrates the market concentration in Manta and Tacna, the largest districts in each region. A student living in the city center of Tacna has access to a broader set of school options compared to peers in Manta. Appendix Table A11 places the two pilot regions beside Chile, nationally and for its seven largest urban areas: the distribution of simulated non-placement risk is similar in Manta, Tacna in 2021 and Chile, with a mean of 0.22 to 0.24 and a quarter to a third of applicants above 0.30 in each (lower in Tacna in 2022: a mean of 0.15 and 22 percent), while the two pilot regions differ in the options within reach and in list length.

¹¹Shares computed from the application data; in Ecuador the share falls only from 61% on the initial list to 59% on the final one.

Figure 1: Schooling Options in a 2 km Radius of the City Center



Notes: The star in Panels (a) and (b) reflects the location of the median student, around which the 2 km radius is drawn. The squares and diamonds represent the off-platform and on-platform schooling options within that radius.

Finally, the interventions were not identical in the two countries.¹² In Ecuador, the warning related to placement risk was not sent as a separate message via WhatsApp, and it appeared on the report card only for the 994 participants (49% of the Ecuadorian experimental sample) with simulated non-placement risk of at least 1%; the other 1,027 had risk below 1%, most of them exactly zero. Applicants in Peru received a specific WhatsApp warning message, which was also included in the report card except in the 2022 baseline arm.

4 Intervention Design

The conceptual framework identifies two frictions that may limit families' application portfolios: costly search for new options and biased beliefs about admission probabilities. Building on successful implementations in Chile ([Arteaga et al., 2022](#)), we designed an information intervention to address both frictions.

¹²Section 7 considers this difference together with delivery reach, search behavior and market density.

The intervention included personalized feedback on non-placement risk and, in some treatment arms, a curated list of nearby schools that families could consider adding to their applications. Each report card was tailored to the applicant’s current application, grade, and geographic location.

Our research team worked with the MoE in both countries to estimate applicants’ non-placement risk. In Peru in 2021, the ministry permitted outreach to applicants with a strictly positive predicted probability of non-placement in the grades the outreach covered (PPK and grade 1). Not all of them were contacted: 1,079 applicants with positive baseline risk in those grades were not, most of whom had applied after the outreach window or had zero risk on the ministry’s send-time score (Section 6.3). In Ecuador, the experimental sample is the 2,021 applicants on the platform when the cards were generated who had no sibling or disability priority, had at least seven schools that could be suggested, and whose guardian had no other applicant; later applicants belong to the universe of 3,984 but not to the experiment. They were randomized regardless of predicted risk, and the report card carried the warning only for the 994 of 2,021 participants (49%) whose simulated non-placement risk was at least 1%; the other 1,027 had simulated risk below 1%, 932 of them exactly zero. In Peru in 2022, the warning was restricted to applicants above a 30% threshold, which governed the content of the report card rather than who was contacted.

Applicants with positive predicted risk were randomized and contacted on both sides of that threshold, but only those above it received the non-placement warning. Every arm above the threshold received it as a separate WhatsApp message. In the list arms, it also appeared on the report card alongside the risk gauge and suggestion list; in the baseline arm, the card summarized the filed application. Applicants below the threshold received only a card summarizing the application they had already filed, with none of these elements and no separate warning message.

Table 1 describes the 1,140 applicants above the threshold in the Peru 2022 experimental sample. The 473 applicants below the threshold form the low side of the discontinuity examined in Section 6. Before the end of the application process, we sent targeted parents information about the chance that their child

might not be assigned to any of their choices.¹³ We randomly assigned applicants to report cards that varied whether they included a suggestion list and, in one arm, additional popularity and congestion information:

T_1 : Baseline report card without a suggestion list

T_2 : Report card + list with up to 10 suggestions

T_{3-2021} : Report card + list with up to 10 suggestions + information on popularity and congestion

T_{3-2022} : Report card + list with 3 suggestions

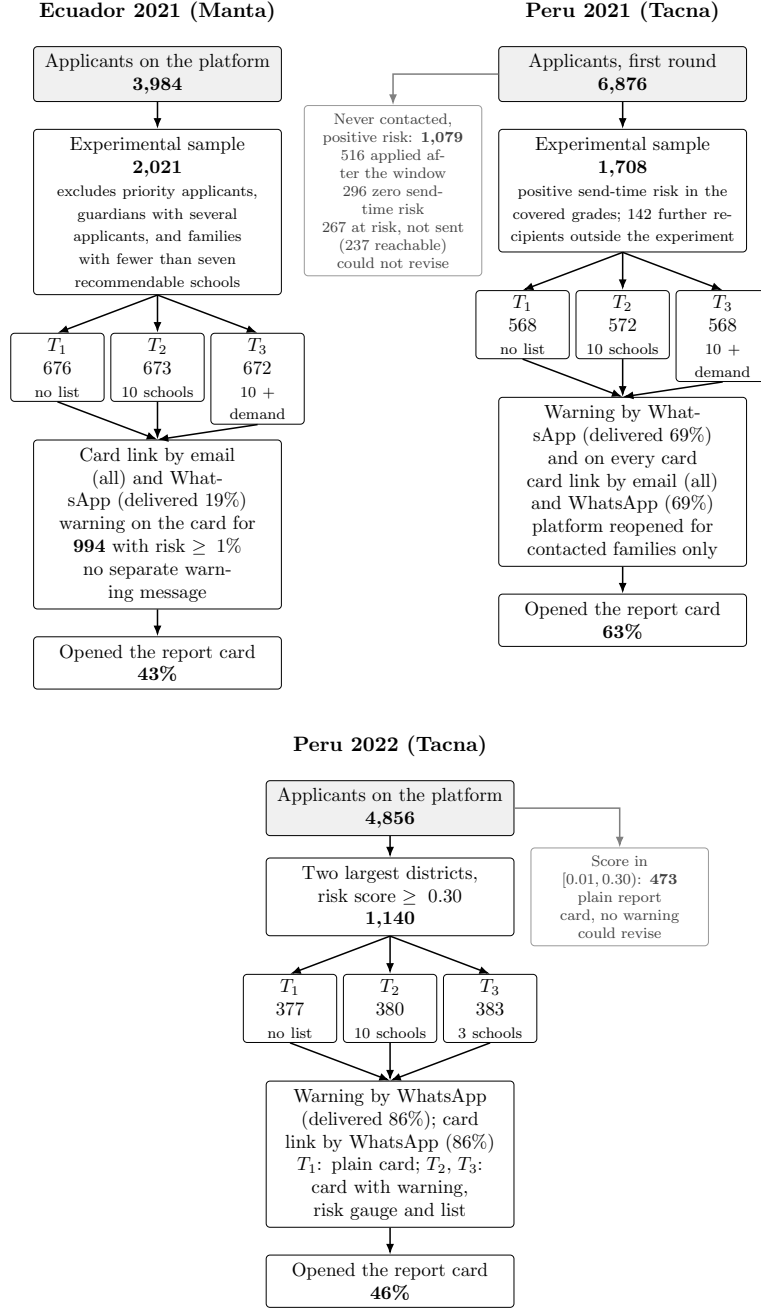
The exact delivery channel and whether the warning appeared separately on WhatsApp, on the report card, or both varied by country and year; Appendix Table A1 summarizes these details, and Figure 2 charts the populations, experimental samples, arms and delivery of the three waves. Assignment was stratified on grade, district, baseline risk bin and gender; Appendix Tables A3 and A4 show the arms balanced on baseline covariates, both for all assigned applicants and for those who opened the report card.

Providing information about available options (T_2 and both T_3 arms) is designed to reduce search costs for marginal applicants. Gathering this information is costly. At least 84% of respondents value details on academic performance, extracurricular activities, and infrastructure, while most also value references, staff interviews, and school web pages (Appendix Figure A5). In addition, 46% of Peruvian and 60% of Ecuadorian respondents consider it important to know which families attend a school.

The intervention facilitates rather than eliminates search, targeting families who marginally stopped looking. Within T_{3-2021} , popularity signals what other families like and congestion signals where applying is safer; because the two were not randomized separately, we cannot distinguish their effects.

¹³In Tacna, the information was delivered four days before the end of the process. In Manta, most report cards went out on February 26, 2021, four to five days before the platform closed, and a reminder followed on March 1. Report cards generated on February 24 and 25 carried arm assignments that were later discarded; Appendix C bounds their influence.

Figure 2: Populations, Samples and Arms in the Three Waves

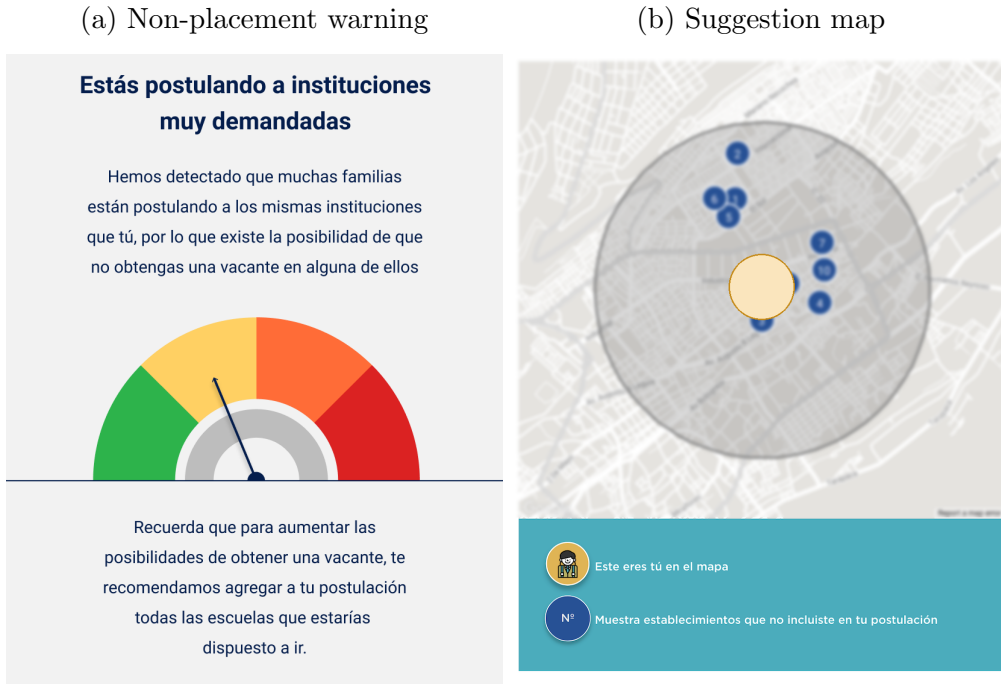


Notes: Counts from Tables 1 and 2 and Sections 4 and 6. Delivery shares are the proportions of the experimental sample whose WhatsApp message was delivered; opening is measured from the report-card page analytics. In Ecuador the warning appeared on the report card of the 994 applicants with simulated risk of at least 1 percent, and no separate warning message was sent. In Peru 2021 only contacted families could revise, and the 142 recipients outside the experiment received the same report card. In Peru 2022 arms were assigned to all applicants with a positive score, but the warning, the gauge and the lists appeared only at or above 0.30.

4.1 Details on Inputs and Construction

For the warning, we used the same message as [Arteaga et al. \(2022\)](#), augmented with a “fire rating” symbol showing the level of risk. The suggestion lists contained up to 10 schools (three in T_{3-2022} ; 23% of Ecuadorian lists had fewer than ten) that the student had not included in the initial ROL, most within 3 km of the declared home address (77% of entries in Ecuador and 91% in Peru 2021), with the T_{3-2021} list constructed to span popular and non-popular, over- and under-subscribed schools. Report cards showed each school’s name, distance, and levels offered, and T_{3-2021} added demand and seat-availability information. Figure 3 shows the warning and the suggestion map as displayed on the report card; the exact messages, construction rules and popularity definitions appear in Appendix D.

Figure 3: Report Card Extracts



Notes: Panel (a) shows the non-placement warning on the Ecuadorian mobile report card; panel (b) shows the map of suggested schools not initially included in the applicant’s ranking, from a Peru 2022 T_2 report card. The full report card is presented in Figure A6 in Appendix A. In panel (b) the applicant’s home location has been redacted: the shaded disc covers an area of roughly 600 meters around the household, street labels have been suppressed, and suggested-school markers falling inside the disc are not shown.

4.1.1 Sample

Non-placement risk was estimated from all applications filed up to the intervention date, and eligible applicants were randomized across the three arms. Assignment determined the content of the report card; delivery was a separate step. In Peru we attempted WhatsApp contact with every applicant in the sample in both years; families who answered the introductory message then received the separate warning message, followed by the report-card link. In Ecuador the contact roster yielded a usable number for only 22% of assigned applicants, so the report-card link reached 19% of them on WhatsApp and the remainder only by email.¹⁴

In Peru in 2021, the online platform allowed families to submit only one application. The authorities therefore provided families assigned to treatment with additional system access so that they could log in and revise their applications. This arrangement distinguishes Peru 2021 from Ecuador 2021 and Peru 2022, where all applicants could revise their lists freely.

The experimental samples are summarized in Table 1. Columns 2, 5, and 8 describe the RCT populations in Ecuador, Peru 2021, and Peru 2022, respectively; all Peruvian participants faced some predicted placement risk, as did 54% of Ecuadorian participants, and 49% of Ecuadorian participants had a simulated non-placement risk of at least 1%, the cutoff for the warning on their report card. The study was designed to treat 51% of applicants in Ecuador, 25% in Peru in 2021, and 23% in Peru in 2022. These shares are not comparable measures of congestion. In Peru in 2021, roughly half of all applicants faced positive risk, but the experiment covered only applicants to PPK and grade 1. In 2022, it extended to all grades.¹⁵ Relative to the average applicant, those assigned to treatment filed shorter pre-intervention portfolios and were more likely to live in the largest districts, Tacna in Peru and Manta in Ecuador. In Peru in 2022, the latter difference is partly mechanical because the intervention was limited to the region's two largest districts, where the report-card congestion and popularity

¹⁴In Ecuador 2021 and Peru 2021, the link was also sent by email; Table A1 in Appendix B details outreach by wave, and Table 2 reports reach by channel.

¹⁵The 2021 experiment did not cover PK and K, so the 2021 experimental sample, and therefore every 2021 estimate in this paper, excludes them. The delivery roster shows that 184 PK and K applicants (160 and 24), 157 of them with positive baseline risk, were nonetheless sent report cards outside the experiment; they enter no analysis here.

measures were available.

4.2 Delivery of Information and Treatment Take-up

We used the WhatsApp messaging app to distribute the links to the report cards with the information for each treatment arm.¹⁶ In our first message, we told parents that we had information about the application to share with them, and asked if they were interested. For those that answered positively, in Peru, we sent a warning about the chances of non-placement followed by a link to the personalized report card. In Ecuador, we only sent the link.¹⁷

Table 2 presents the main statistics on the intention to treat and messaging reception. Panel B shows that WhatsApp reached far more applicants in Peru. We sent an introductory message to 100% of the applicants assigned to the treatment, and 89% of them read it in the 2021 version and 92% in the 2022 version.¹⁸ In Ecuador, we sent WhatsApp messages to only 22% of the targeted population, and 90% read them.¹⁹

Repliers were then sent the report-card link (panel D), preceded in Peru by the warning message (panel C): 69% and 86% of Peruvian treated applicants in 2021 and 2022 received both the warning and the link, while in Ecuador 19% received the link and no separate WhatsApp warning was sent (panel C is zero throughout); read rates closely matched sent rates in this already-responsive group.

¹⁶In both countries in 2021, we also sent the information by email. Table A1 contains a summary of the interventions and channels.

¹⁷In Peru 2021, the warning was included in the report card for all arms. In Ecuador 2021, it was included in every arm's card, but only for applicants whose simulated non-placement risk was at least 1%. In Peru 2022, all arms received the separate WhatsApp warning, while the warning also appeared in the report cards for the list arms. For more details on the messages, the original text, and translations to English, see Appendix C.

¹⁸A particular feature of the WhatsApp messaging app is that it provides insights into message status since it distinguishes between messages that have been sent, delivered, and read.

¹⁹The low rate of messaging in Ecuador was not by design. Among applicants to whom a message was sent, 97% had it delivered and 90% read it, rates at or above Peru's (92 and 89% in 2021; 96 and 92% in 2022); no message was ever dispatched to the other 78% of the sample, because the administrative records did not yield a usable number for them. Our data cannot say why the roster was short: numbers absent, invalid, fixed-line, or not cleared for contact are observationally equivalent here. Reached and unreached applicants look alike on what we observe (baseline risk 0.27 against 0.25, $p = 0.21$), so this is a coverage gap rather than selection on risk.

Table 2: Take-up of WhatsApp Messages and Report Card

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Ecuador 2021				Peru 2021				Peru 2022			
	In RCT	Treatment			In RCT	Treatment			In RCT	Treatment		
		T_1	T_2	T_3		T_1	T_2	T_3		T_1	T_2	T_3
<i>A. Email with link to report card</i>												
Sent	1	1	1	1	1	1	1	1	0	0	0	0
<i>B. WhatsApp introduction</i>												
Sent	0.22	0.21	0.21	0.25	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Delivered	0.21	0.20	0.19	0.25	0.92	0.94	0.93	0.91	0.96	0.96	0.96	0.96
Read	0.20	0.19	0.18	0.23	0.89	0.90	0.88	0.88	0.92	0.92	0.92	0.92
<i>C. WhatsApp warning</i>												
Sent	0.00	0.00	0.00	0.00	0.69	0.68	0.72	0.67	0.86	0.85	0.88	0.86
Delivered	0.00	0.00	0.00	0.00	0.69	0.68	0.72	0.67	0.86	0.85	0.88	0.86
Read	0.00	0.00	0.00	0.00	0.66	0.66	0.68	0.65	0.83	0.83	0.84	0.81
<i>D. WhatsApp with link to report card</i>												
Sent	0.19	0.18	0.16	0.22	0.69	0.68	0.72	0.67	0.86	0.85	0.88	0.86
Delivered	0.19	0.18	0.16	0.22	0.69	0.68	0.72	0.67	0.86	0.85	0.88	0.86
Read	0.17	0.17	0.14	0.19	0.66	0.66	0.68	0.65	0.83	0.83	0.84	0.81
<i>E. Opened link of report card (Google Analytics)</i>												
Link delivered by WhatsApp	0.76	0.73	0.79	0.77	0.71	0.66	0.72	0.75	0.53	0.56	0.48	0.53
Link not delivered by WhatsApp	0.36	0.32	0.35	0.41	0.46	0.48	0.44	0.46	0.05	0.04	0.04	0.07
All	0.43	0.39	0.42	0.49	0.63	0.60	0.64	0.65	0.46	0.48	0.43	0.47
N	2,021	676	673	672	1,708	568	572	568	1,140	377	380	383

Notes. All statistics are proportions in the population defined by the column header. Panels B to D show sent, delivered and read status for the three WhatsApp messages; Panel A shows only whether an email was sent, because the mail platform returned no reception data. “Sent” means that we tried to reach the applicant, “Delivered” that the applicant received the message on their device, while “Read” that the applicant saw the message. We sent additional WhatsApp messages only to applicants who answered positively to the first one, the “WhatsApp introduction” (Panel B). The “WhatsApp warning” (Panel C) contained the placement-risk alert and a recommendation to add schools; the “WhatsApp with link to the report card” (Panel D) followed it and carried the link to the personalized report card. In Panel E, the first row is conditional on the link being delivered through WhatsApp, the second on not being delivered, while the third row is unconditional.

Email links (used in 2021 only, panel A) mattered most in Ecuador: report-card view rates were 43, 63, and 46% in Ecuador 2021, Peru 2021, and Peru 2022 (panel E), and among applicants never reached on WhatsApp, 36% still opened the report card in Ecuador 2021 and 46% in Peru 2021, versus just 5% in Peru 2022. The differential delivery reach across contexts, which in Ecuador reflects a short contact roster and not the messaging channel, scales down the intention-to-treat estimates where reach was low, but it does not explain the null of the

recommendation arms in Ecuador, which holds among families who opened the report card (Section 6).

Report-card content is seen only by families who open the card, and the list appears only inside it. Opening rates do not differ significantly between T_1 and T_2 in any wave: they are 0.39 and 0.42 in Ecuador, 0.60 and 0.64 in Peru 2021, and 0.48 and 0.43 in Peru 2022 (Table 2, Panel E). The T_{3-2021} arm opened the card more often than T_1 : by 9 percentage points in Ecuador, a significant difference of which higher WhatsApp coverage in that arm (Panel B) accounts for only about 2 points, the rest coming from higher opening among applicants without a delivered link (41 against 32 percent), which the data do not explain; and by 4.9 points in Peru 2021 ($p < 0.10$). Among openers, baseline covariates are balanced across arms (joint p -values between 0.37 and 0.98; Appendix Table A4). We report two estimands and lead with the first. The intention-to-treat effect on all assigned applicants is the effect of the policy as delivered. The effect among families who opened the report card describes the families the card reached; it is a valid comparison if assignment did not change who opened the card, which the opening rates and the balance among openers support except in the Ecuadorian T_{3-2021} arm, for which we rely on the intention-to-treat estimate. Because no T_1 card carries a list, dividing the intention-to-treat effect of T_2 by the T_2 opening rate gives the Wald estimate of the list’s effect on families who open a card that carries it. That estimate assumes that assignment does not affect families who do not open the card. In Ecuador, applicants recorded as not opening it show a $T_2 - T_1$ difference of 0.014 (s.e. 0.009) in adding a listed school, so we treat the rescaling as descriptive (Appendix Table A9).

4.3 Pre-analysis Plan and Exploratory Analyses

We distinguish between analyses specified in the pre-analysis plan and analyses developed subsequently. The plan is dated February 2021, and the trials were registered in the AEA RCT Registry (AEARCTR-0007122) on March 1, 2021. Registration therefore followed the Peru 2021 wave, fielded in January 2021, and came while the Ecuador 2021 wave was under way: its report cards were sent between February 25 and March 3, 2021, most of them on February 26. Only the

Peru 2022 wave was fielded entirely after registration. The plan specified two primary intention-to-treat outcomes, the number of schools listed and the probability of assignment through the platform. It also specified four secondary outcomes, including whether an applicant added a school displayed on the personalized report card.

Neither primary outcome, the number of schools listed (measured as net schools added) or placement through the platform (measured as simulated non-placement risk), shows an intention-to-treat effect of the suggestion lists significant at the 10% level in any wave (Table 7). Realized first-round non-placement, a measure of placement the plan did not specify, is significant only for T_2 in Peru 2021, and only at the 10% level (-0.046 , s.e. 0.026). Our central outcome, adding a suggested school, is the first pre-specified secondary outcome. In Peru 2021, assignment to the ten-school list raised it by 6.3 percentage points (s.e. 2.4) and assignment to the T_{3-2021} list by 7.4 (s.e. 2.4), both significant at the 1% level; in Ecuador the ten-school estimate is 1.2 percentage points (s.e. 1.1).

Several analyses reported in the paper were not specified in the plan and should therefore be interpreted as exploratory. They include analyses restricted to families who opened the report card, pooled specifications across cohorts, realized rather than simulated non-placement, comparisons with never-contacted applicants, and the Peru 2022 wave. The plan also proposed a difference-in-differences analysis among Ecuadorian applicants with zero predicted risk and a regression discontinuity across the four displayed risk categories; we do not report either. Finally, rather than the planned interactions with characteristics of suggested schools, we examine heterogeneity by baseline risk.

5 Survey

We conducted an online survey to elicit participants' beliefs about placement probabilities, their level of knowledge about schooling options, and the difficulty of the application process. The surveys were distributed once the application processes had closed, but before the placement results were made public. The evidence shows limited awareness and biased beliefs, the frictions the intervention targets.

5.1 Survey Design and Implementation

Our questionnaire aims to gain a better understanding of participants’ knowledge and beliefs relating to the application process.²⁰ We included novel questions on parents’ understanding of the mechanism, interpretation of school popularity, and awareness of private (off-platform) options. We distributed links to the survey (on the Qualtrics platform) by WhatsApp and, in Ecuador, also by email, to every applicant on each platform; the email channel is why survey coverage in Ecuador (47%) exceeds the share of applicants reached on WhatsApp (22%).

The survey was personalized to each applicant’s submitted ROL and home location, covering perceptions of the platform, application strategy, awareness of on- and off-platform options nearby, beliefs about assignment probabilities, and satisfaction with hypothetical placements.

Fielded after applications closed but before results were public (so placements could not move beliefs), the survey sampled one child per household, was incentivized apart from a one-day reminder, and reached completion rates of 25% and 31% in the two Peruvian years and 47% in Ecuador, 32% of all applicants pooled (Table 1, columns 3, 6, and 9), against 11% for the 2020 Chilean survey on which the questionnaire is based (48,929 of about 450,000 applicants; [Arteaga et al., 2022](#)). Three features of the fielding likely explain the difference, although we cannot separate them. The invitation came from the ministry, on the channel through which families in the experiment had just received their report card, so the sender was known and the link opened in one tap. It arrived in the days between the application deadline and the publication of results, when families were waiting for exactly the information the survey asked about. And the questionnaire was short and personalized to the family’s own application. Completion was highest in Ecuador, where the links also went by email, the channel through which every family in the experiment had received the report card. Respondents skew toward the largest districts, slightly denser school zones, and longer applications; they are representative in grade and gender.

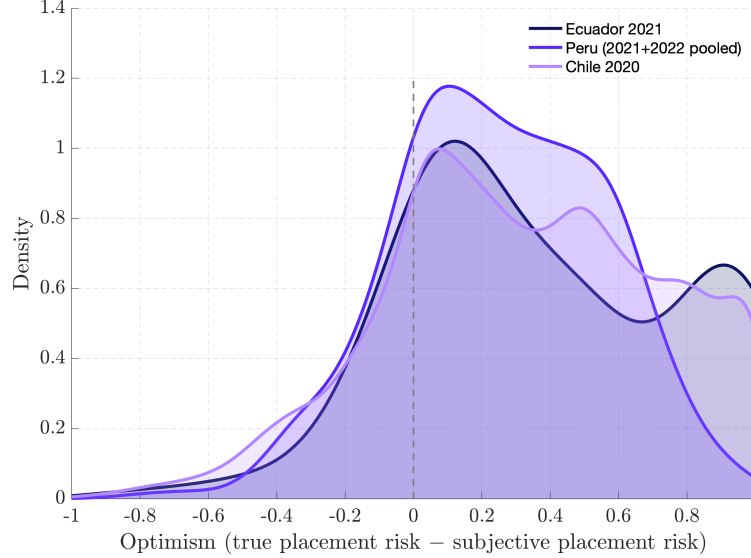
²⁰The content of the survey is based on the questionnaire of [Arteaga et al. \(2022\)](#).

5.2 Survey Evidence on Model Frictions: Cross-Country Comparisons

The conceptual framework in Section 2 identifies two frictions: biased beliefs about admission probabilities (optimism parameter a) and costly search over a limited consideration set (search cost C and initial awareness C_0). Our survey measures both and compares their magnitude across Peru, Ecuador, and Chile ([Arteaga et al., 2022](#)). Finding the same frictions in three countries with different school systems and markets supports the expectation that the warning transports.

We begin with the first friction, biased beliefs about placement probabilities. Figure 4 shows the distribution of optimism bias across Peru, Ecuador, and Chile. The distributions are centered on substantial overestimation of placement probabilities in all three countries, evidence that the optimism parameter a in our framework is large in each of these Latin American contexts. All three distributions are centered well above zero; their levels are not equal. For the two surveys we fielded, a Welch test rejects equal means (25 percentage points in the pooled Peruvian sample against 37 in Ecuador, a difference of 12 points with a standard error of 3) and a Kolmogorov-Smirnov test rejects equal distributions ($p < 0.001$), and the two Peruvian waves also differ (29 against 20 points; Appendix Table A10). We do not test Chile, whose instrument, timing, and sampling frame differ. What transports across the three countries is therefore the sign and the order of magnitude of the bias, not its exact level.

Figure 4: Distribution of Optimism Bias

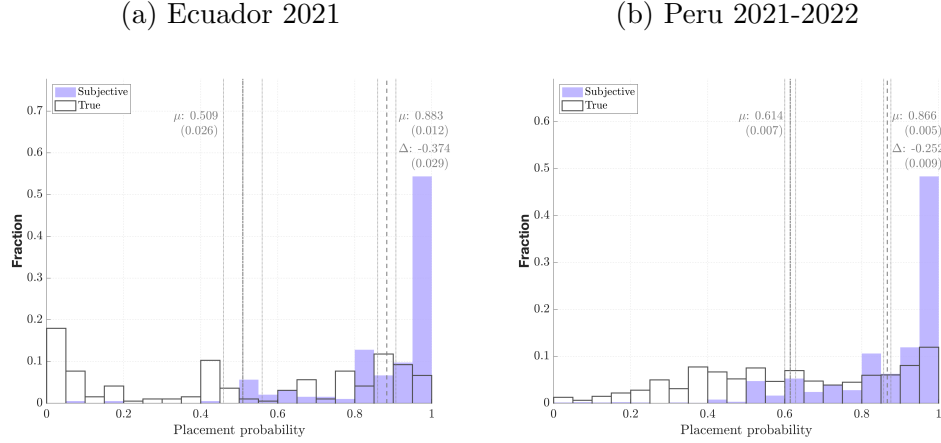


Notes: Figure shows the kernel density for the distribution of the differences between the subjective and true placement probabilities for the subset of applicants with placement risk > 0.01. The subjective placement probability comes from the question “On a scale from 0 to 100, with what probability do you think that [applicant name] will obtain a spot in at least one of the [number of schools in ranking] schools in the ranking?” Peru pools the 2021 and 2022 waves; the Chilean distribution is from the 2020 survey of applicants reported in [Arteaga et al. \(2022\)](#).

Quantitatively, applicants with positive non-placement risk hold beliefs that are 20-37 percentage points more optimistic than true placement probabilities (Figure 5). In Ecuador, the average optimism bias is 37pp; in Peru, it is 29pp in 2021 and 20pp in 2022, 25pp in the pooled Peruvian panel the figure shows. The magnitudes are comparable to Chile ([Arteaga et al., 2022](#)), so high optimism (a) is common to these markets. Extreme optimism (a bias of more than 80pp) is concentrated in Ecuador (Panel 5a): about a quarter of Ecuadorian respondents with positive non-placement risk (24%), against about 3% in Peru.²¹ Despite this tail difference, optimism is large and positive in every context, and highest in Ecuador.

²¹This difference may reflect Peru’s prior experience with decentralized competitive admissions, while Ecuador’s previous system assigned students to the closest school based on location.

Figure 5: Distribution of Subjective and True Placement Probabilities



Notes: Panels (a) and (b) show the distribution of subjective placement probability (light purple, filled bars) and true placement probability (dark grey, outlined bars). Vertical lines indicate means with standard errors, and Δ represents the difference between true and subjective means.

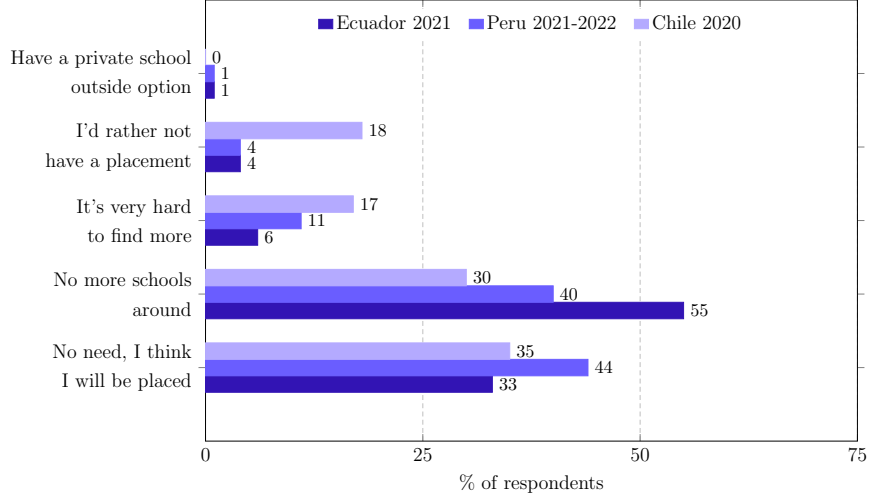
The second friction is limited awareness and costly search. The framework predicts that high optimism reduces the perceived value of search, while high search costs and limited initial awareness constrain portfolio expansion. Figure 6 reports families' stated reasons for stopping search. Across Peru, Ecuador, and Chile, the dominant reasons are optimism ("I think I will be placed") and limited available options ("No more schools around"). In Peru, optimism is the modal response (44%), consistent with the high measured a . In Ecuador, limited availability dominates (55%), consistent with lower school density. In Chile the optimism share (35%) lies between the two, but fewer families cite a lack of nearby schools (30%) and more say they would rather go unplaced or find search hard (18 and 17%). These responses describe why families report stopping search rather than providing a direct comparison of belief bias across countries. The prominence of limited alternatives in Ecuador suggests that correcting beliefs may leave less scope for further search, even when optimism is substantial. These are the two conditions in the diagnostic of Section 2.4: optimism gives the warning something to correct, and a lack of alternatives leaves families nothing to add. Ecuador meets the first condition at least as strongly as Peru on measured optimism but fails the second more often, which the framework predicts should attenuate the

response. Section 7 shows, however, that market density alone does not order the list effects across waves.

The same question speaks to the outside option, which equation (4) shows is otherwise hard to separate from optimism. The two fallback responses account for only 6.0%, 3.9%, and 5.6% of answers (Peru 2021, Peru 2022, Ecuador): one family in twenty stops searching because it has an acceptable fallback, against a third to a half who expect to be placed. Asked how they would regard ending up with no placement (a stated counterpart to u_0), more than 90% choose the bottom half of the 20-point scale everywhere, though the share giving it the lowest possible score is lower in Ecuador (42.7%) than in Peru (58.4% in 2021 and 50.2% in 2022).²² Short lists therefore reflect beliefs and search costs more than acceptable fallbacks, which is why information can lengthen them.

²²Administrative records show what the fallback is in practice: among applicants unplaced through the platform in Peru 2021, 78% are later observed enrolling, 82% of them at an on-platform school through a later round or direct vacancy (18% off-platform); in Ecuador, 95% and 90%. The outside option is thus predominantly a delayed placement within the same public system. Enrollment coverage is incomplete and differential (90% for placed vs. 78% for unplaced Peruvians), so the residual should not be read as dropout.

Figure 6: Reasons for Not Adding More Schools



Notes: Panels represent the answer to the question “Why didn’t you add more schools to your application? (select the main reason)” among all survey respondents who answered it (1,639 in Ecuador, 1,382 in Peru 2021 and 1,185 in Peru 2022). Peru bars average the 2021 and 2022 waves. Chile bars are from the 2020 survey of Chilean applicants reported in [Arteaga et al. \(2022\)](#).

We also asked families whether they knew, at least by name, each of up to five schools drawn from the family’s suggestion list: schools near home, most of them within 3 kilometers, that the family had not applied to. Among respondents who opened a report card that did not show the list (T_1), the share known was 36% in Ecuador, 36% in Peru 2021 and 21% in Peru 2022 (Table A8, Panel C), between a fifth and a third of these nearby alternatives. This limited awareness corresponds directly to the small initial consideration set C_0 in the model. When combined with high search costs (documented in Appendix Figure A5, showing that more than 84% of families value detailed information that is costly to obtain), these frictions explain why portfolios remain short even when non-placement risk is high.

The survey also bears out the framework’s assumption that any placement beats none: more than 90% of families give non-placement a failing grade, while 90% grade their first preference excellent (Appendix Figure A7, Panels A7a and

A7b). Satisfaction declines down the list, but even the last-ranked school is strongly preferred to non-placement in both countries.

Finally, we document that families are not well-informed about private (off-platform) schooling options in their neighborhood. Panels A3a and A3b show that more than 40% of applicants have never heard of the largest private school within 3 km of their home, rising to more than 60% for the closest private school. As a benchmark, only 4–10% of applicants lack knowledge of their first-ranked school, confirming that unfamiliarity reflects limited search rather than general inattention (Figure A4 in Appendix). This pattern of limited off-platform awareness is consistent across Peru and Ecuador, suggesting that information frictions extend beyond the public school sector.

Two additional survey patterns help interpret the mechanisms behind the intervention. Many families mistakenly believe that adding schools to their list can reduce their chances at higher-ranked options (Appendix Figure A1). This misunderstanding creates a perceived cost of portfolio expansion and therefore works against the behavioral response the warning is designed to encourage. The pattern is consistent with evidence from laboratory deferred acceptance and the medical residency match showing that participants often misreport because they misunderstand strategy-proof mechanisms rather than because they attempt to strategize (Guillen and Hing, 2014; Rees-Jones and Skowronek, 2018); Rees-Jones (2017) works out the welfare implications of such mistakes under deferred acceptance. Even applicants who understand the ranking incentives may omit attractive safety options when admission outcomes are correlated (Rees-Jones et al., 2024).

Families also do not interpret school popularity uniformly. Only about a fifth read popularity as a sign of quality; similar shares call it uninformative or say they would need to check the school, and in Ecuador 41% do not know (Appendix Figure A2). This heterogeneity suggests scope for interventions that pair risk feedback with clearer guidance on the mechanism and on school-level signals, although we do not test those extensions here.

In all three countries, then, optimism is large, families know few of the nearby schools their report cards list, search is costly, and non-placement is rated far below any placement. Whether correcting these frictions changes behavior also depends on the alternatives available and on families' ability to act on the infor-

mation, which Section 6 examines.

6 Choice Behavior and Choice Outcomes

This section shows that warning families about their placement risk changed their applications and their placement, replicating the Chilean evidence (Arteaga et al., 2022), and then reports the exploration of what recommendations add that our pre-analysis plan set out. Section 6.1 presents the 2022 risk threshold, the same design as the Chilean study. Section 6.2 compares families who received the warning with families who did not. Section 6.3 turns to placement, for warned families and for the market as a whole. Section 6.4 reports the randomized suggestion lists, and Section 6.6 discusses why the response was weaker in Ecuador.

6.1 Warnings and Applications at the 2022 Threshold

We first examine whether crossing the 30% risk threshold changed applicant behavior in Peru 2022, as the framework predicts when families receive information about non-placement risk. Figure 7 juxtaposes the Peru 2022 regression discontinuity with the pooled Chilean estimates from Arteaga et al. (2022, Figure 5b). In both settings, predicted placement risk determined what a family was shown and appears on the horizontal axis. The two panels, however, estimate different treatment contrasts.

In Chile, only the pop-up warning switched on at the cutoff. In Peru 2022, the threshold governed the full report card. Below it, families received a summary of their submitted application; above it, families in the list arms received risk feedback, a risk gauge, a map of nearby options, and a suggestion list, as well as a separate WhatsApp warning.²³ The discontinuity therefore compares the full information package with a card containing none of these elements among list-arm applicants whose report-card link was delivered ($N = 939$; 277 below and 662 above the cutoff).

The jump is positive: families just above the threshold added about a third

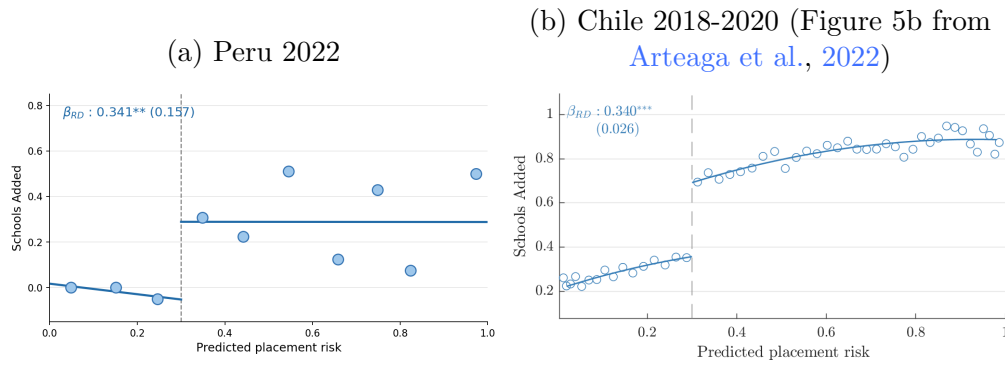
²³The T_1 card carried none of these elements on either side of the threshold, so T_1 families above it received the warning only by WhatsApp.

of a school more than those just below (0.34 schools, s.e. 0.16), significant at the 5% level under the uniform-weighted fit drawn in the figure, which fits separate linear trends over the entire risk support; the robust bias-corrected estimate for the same fit is 0.40 (s.e. 0.28). The estimate is not precise at any local bandwidth, however: at the MSE-optimal window the point estimate is larger (0.80 schools, s.e. 0.77) but far from significant, and no estimate of schools added at fixed bandwidths between 0.05 and 0.30 approaches conventional significance, although the share adding any school is significant at a bandwidth of 0.30 (0.078, s.e. 0.035). Appendix G reports the full bandwidth path and placebo cutoffs. We report no discontinuity estimate for placement risk in this wave, because those estimates are indistinguishable from zero at every bandwidth narrower than 0.4 and become significant only when the window covers most of the support and the fit is a slope rather than a jump.

Table 3 separates the arms and uses the outcome that varies most sharply at the threshold, whether the family added any school. Among list-arm families, whose report card above the threshold carried the warning and the list, the share adding a school jumps by 6 to 8 percentage points at every bandwidth from 0.10 to 0.30, for example by 7.7 percentage points (s.e. 3.0) at a bandwidth of 0.30 among all assigned list-arm applicants, although robust bias-corrected inference does not reject zero at any bandwidth, and a placebo cutoff at 0.15 shows a small jump in the same outcome (0.016, significant at the 5% level; Appendix G). The score itself shows no bunching at the cutoff: the density test of Cattaneo et al. (2020) does not reject continuity for all applicants ($T = -0.26$, $p = 0.79$), for the list arms ($p = 0.33$), or for T_1 ($p = 0.41$; Appendix Table A18). Families did not know the threshold, and the score was computed by the ministry from the submitted application, so sorting would have had to come through the score’s construction. Families in the arm without a list, who received the warning only as a WhatsApp message and a report card that carried neither the warning nor a list, show no jump: their estimates are close to zero or negative at every bandwidth. The two arms’ jumps are not statistically distinguishable, however: their difference is 0.09 to 0.11 at every bandwidth, with p -values between 0.10 and 0.40 (column 4), and the randomized contrasts above the threshold show no difference between the list arms and T_1 in the share adding any school (-0.001 and 0.018 , s.e. 0.021;

Table 7). Delivery of the link, opening, gender and district show no discontinuity at the threshold among list-arm applicants; the grade mix differs across the cutoff at the wide bandwidth, as it should when grade composition changes with risk, and adding the covariates to the local linear fit leaves the jump at 0.078 (s.e. 0.030) (Appendix Table A17). Figure 8 shows the pattern behind these estimates. Among list-arm families the share adding a school is close to zero just below the threshold and steps up at it; in the arm without a list it is also close to zero below the threshold but rises only gradually with risk above it, so that the fits on the two sides meet at the cutoff. In 2022 the response at the threshold therefore appears in the arms whose report card carried the warning, which also carried the list, and not detectably in the arm warned by WhatsApp alone, although the arms' estimates are not statistically distinguishable; the 2021 evidence below shows that a report card carrying the warning without a list moved families too. The discontinuity points the same way as the Chilean estimate. Because it is imprecise in narrow windows, we read it together with the revision and placement evidence that follows.

Figure 7: Schools Added RD



Notes: Binned means and global fits of schools added after the information campaign by predicted risk for the pre-campaign application. The non-placement warning was assigned only to applicants with a predicted risk higher than 0.3 (30%), as indicated by the vertical dashed line. The Peru 2022 panel is estimated on T_2 and T_3 -2022 applicants whose report-card link was delivered ($N = 939$; 277 below the threshold and 662 above). Above the threshold these families received the warning, the risk gauge, a map and the suggestion list; below it they received a card with none of these, so the jump reflects the package, not the warning alone. Delivery is not randomly assigned, so this is a comparison among families the card reached. Note that the y-axis scales differ across panels: the Peru 2022 axis runs from about -0.15 to 0.85 schools, because one binned mean is negative, while the Chile 2018–2020 axis runs from about 0.1 to 1.1 .

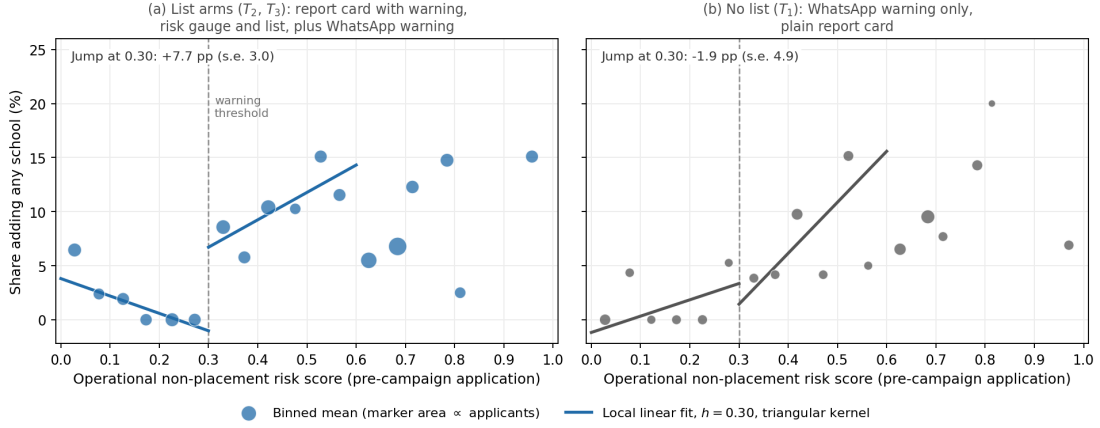
Table 3: The 2022 Threshold by Arm: Share Adding Any School

Bandwidth	(1) List arms, link delivered	(2) List arms, all assigned	(3) No list (T_1): WhatsApp warning only	(4) Difference (2) – (3)
$h = 0.10$	+0.070 (0.068) [0.69] $N_h = 205$	+0.070 (0.051) [0.75] $N_h = 237$	−0.024 (0.093) [0.43] $N_h = 97$	+0.094 (0.111) [0.40] $N_h = 334$
$h = 0.15$	+0.064 (0.049) [0.49] $N_h = 311$	+0.070* (0.040) [0.37] $N_h = 361$	−0.044 (0.080) [0.82] $N_h = 164$	+0.114 (0.092) [0.22] $N_h = 525$
$h = 0.20$	+0.061 (0.043) [0.37] $N_h = 389$	+0.066* (0.037) [0.21] $N_h = 452$	−0.041 (0.068) [0.65] $N_h = 209$	+0.107 (0.079) [0.18] $N_h = 661$
$h = 0.30$	+0.078** (0.035) [0.36] $N_h = 571$	+0.077*** (0.030) [0.18] $N_h = 661$	−0.019 (0.049) [0.54] $N_h = 323$	+0.096 (0.059) [0.10] $N_h = 984$
MSE-optimal ($h \approx 0.14$)	+0.064 (0.049) [0.34] $N_h = 311$	+0.068 (0.043) [0.22] $N_h = 309$	−0.043 (0.082) [0.79] $N_h = 159$	+0.114 (0.097) [0.24] $N_h = 471$

Notes: Regression-discontinuity estimates at the 0.30 cutoff on the operational risk score (local linear, triangular kernel, estimated with `rdrobust`) for the share of applicants who added any school after the report card. Above the cutoff, list-arm families (T_2 and T_{3-2022}) received a report card carrying the warning, the risk gauge and the suggestion list, plus a separate WhatsApp warning; families in the arm without a list (T_1) received the WhatsApp warning only, with a report card that carried neither. Below the cutoff every arm received a plain report card. Column 1 is the sample of Figure 7. Column 4 is the difference between the list-arm and T_1 jumps, from one local linear regression on all assigned applicants within the bandwidth with interactions of side, score and arm (triangular weights, HC1 standard errors and p -value); its arm-specific jumps equal the conventional estimates of columns 2 and 3. Conventional standard errors in parentheses, robust bias-corrected p -values in brackets (for column 4, the HC1 p -value), and the effective number of observations below them. Source: `codeAnalysis/rd_by_arm_peru2022.py`. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We next explain why the design combines a rule for the warning with randomized recommendations rather than randomizing throughout. Although the warning may appear costless from a research perspective, officials did not see it that way. Telling a family that their child is likely to go unplaced can alarm parents, generate complaints, and create reputational risk for an assignment system whose legitimacy was not yet established. The relevant constraint was not concern about withholding information from a control group, but the political cost

Figure 8: The 2022 Threshold by Arm: Share Adding Any School by Risk Score



Notes: Share of Peru 2022 applicants who added any school after the report card, by the operational non-placement risk score of the pre-campaign application. Markers are means within bins of width 0.05 (the top bin pools scores of 0.85 and above), with area proportional to the number of applicants. Lines are the local linear fits on each side of the 0.30 cutoff (triangular kernel) that underlie the $h = 0.30$ row of Table 3, and the annotated jump is the estimate in that row with its conventional standard error. Panel (a) pools the list arms (T_2 and T_3 –2022, all assigned, $N = 1,081$), whose report card above the threshold carried the warning, the risk gauge and the suggestion list in addition to a separate WhatsApp warning. Panel (b) is the arm without a list (T_1 , $N = 532$), which received the warning only by WhatsApp and a report card that carried neither. Below the threshold every arm received a plain report card. Source: `codeAnalysis/rd.by_arm_peru2022.py`.

of sending the message at all.

This shaped the design in two ways. First, the ministry would not allow warning assignment to be randomized. It required a transparent rule that it could defend publicly. In Peru in 2021, the pilot’s first year, the ministry permitted outreach to applicants with any positive placement risk in the grades the outreach covered. In practice, 1,079 applicants with positive baseline risk in those grades were never contacted. Most of them had applied after the outreach window (516) or had zero risk on the ministry’s send-time score; 267 had positive send-time risk, 237 of them with a nine-digit cell number on file, and the delivery files record no reason why these reachable applicants were not contacted (Section 6.3). There was no threshold and therefore no discontinuity, so in that cohort the evidence on the warning comes from comparing warned families with applicants who were never contacted.

The comparison is more informative than a simple before-and-after because the ministry also restricted who could revise: concerned that the platform would

not withstand a wave of simultaneous edits, it reopened the application only for families in the outreach group. Applicants outside that group could not modify their lists at all. Because only contacted families could revise, the never-contacted group’s change in risk measures how much the contacted families’ revisions moved the risk of families who did not revise; we net this drift out below.

For 2022, we proposed a 0.30 threshold, which the ministry accepted in part because the same cutoff was already in use in Chile. Prior adoption lowered the institutional cost of applying the same rule. The two settings therefore share an operational cutoff, although, as noted above, they do not share the same treatment contrast. What could be randomized was the content of the report card: among the applicants who could be shown a list, the presence and length of the suggestion list were randomly assigned. Evidence on the warning therefore comes, as in the Chilean study, from the rule itself and from comparing families who received the warning with families who did not, while randomization was used to explore what recommendations add. We regard this constraint as substantive. An intervention whose perceived political cost rises with reach faces a different scaling problem from one that is merely expensive to deliver.

6.2 Warned Families Revise; Unwarned Families Barely Do

Families had submitted their applications before any warning was sent, and most had no plan to revise them. Each family’s application before and after the report card therefore shows what the information changed for that family. Among Peruvian families in the arm without a list (T_1) who opened the report card, 34% added a school in 2021, a net 0.9 schools per opener, and 13% in 2022, a net 0.3 schools (Table 8, columns 4 and 7). In Ecuador, 15% of the openers whose card carried the warning added a school, against 5% of those whose card did not (column 1).

Families who did not receive the warning show what would have happened otherwise. In 2021, families who were not contacted could not revise their applications at all. In 2022 every family could revise, and the 0.30 threshold separated families who received the warning from families who received a plain report card.

Panel B of Table 4 shows that families below the threshold almost never revised: 1.7% added a school, and their lists became slightly shorter on net. Above the threshold, 9.3% added a school, 7.6 percentage points more than below (s.e. 1.0), a net 0.24 schools. Close to the threshold, between risk scores of 0.25 and 0.35, the shares are 1.4% below and 7.3% above (a difference of 5.9 percentage points, s.e. 3.0). These comparisons are descriptive, because risk differs across the threshold, and the discontinuity in Section 6.1 is the formal comparison. Families in the arm that received only the WhatsApp warning also added schools more often above the threshold than families below it (8.8%), but they show no jump at the threshold itself (Table 3), so we cannot attribute their higher rate to the message rather than to their higher risk. The survey points the same way: in 2022, families in that arm were less likely than list-arm families to say they had received the warning (Section 6.7). In 2022 the response at the threshold is concentrated in the arms whose report card carried the warning, although the arms’ estimates are not statistically distinguishable; in 2021, when every report card carried it, families who received the card without a list revised heavily, as the first paragraph of this subsection shows.

6.3 Warnings and Placement

The warning’s purpose is to keep children from going unplaced. The outcome families experience is whether the child obtained a place in the first round, and it is recorded for every 2021 applicant, not only for those in the experimental sample.²⁴

The 2021 implementation provides the comparison group: the ministry reopened the platform only for the applicants we contacted, so every other applicant with positive predicted risk is observed over the same window without a warning, a suggestion list, or an opportunity to revise. Contacted families received the warning together with the report card and, in two of the three arms, a list; the

²⁴The ministry’s post-assignment file records the first-round assignment outcome of all 6,876 applicants; the pre-assignment file of the same round contains zeros throughout and cannot be used for this outcome. In that file, code 5 denotes no first-round placement. We validate the decoding against the experimental sample, where the placed school is known independently: 565 of the 566 applicants recorded as unplaced there carry code 5, and none of the 1,142 placed applicants does.

Table 4: Warnings, Revisions and Placement in Peru

<i>A. Peru 2021: expected number of children unplaced in the first round</i>				
	Applicants	Before	After	Change
Warned families (experimental sample)	1,708	824.7	684.2	-140.5
Other applicants (could not revise)	5,168	846.9	877.1	+30.2
All round-1 applicants	6,876	1,671.6	1,561.2	-110.3
	Applicants			Change per 100
Warning, no list (T_1)	568			-7.6
Warning and ten-school list (T_2)	572			-8.7
Warning, list and popularity information (T_{3-2021})	568			-8.4
<i>B. Peru 2022: revisions by side of the 0.30 warning threshold</i>				
	Applicants	Added any school	Difference from below	Net schools added
Below 0.30: plain report card, no warning	473	0.017		-0.025
Above 0.30: warned, all arms	1,140	0.093	+0.076 (0.010)	+0.238
Above 0.30: WhatsApp warning only, no list (T_1)	377	0.088	+0.071 (0.016)	+0.210
Within 0.25-0.35, below the threshold	71	0.014		+0.000
Within 0.25-0.35, above the threshold	96	0.073	+0.059 (0.030)	+0.344

Notes: Panel A: expected number of children unplaced in the first round of Peru 2021, the sum of simulated non-placement probabilities from deferred acceptance run on all 6,876 round-1 applications with capacities fixed. “Before” returns the 1,708 experimental families to their pre-intervention lists and “After” uses final lists. Only contacted families could edit their applications in 2021; the 299 recipients outside the experiment are held at their final lists in both scenarios, because their pre-intervention lists were not archived, so the change in the total is the net effect of the experimental families’ revisions given everyone else’s final list, including any displacement of other applicants. The simulation reproduces the official final-round risk (correlation about 0.98). Changes per 100 families are for each arm’s own families. Panel B: Peru 2022 applicants with positive predicted risk, by side of the 0.30 risk threshold; above it families received the warning (the T_1 arm by WhatsApp only, the list arms also on the report card together with a list), below it a plain report card. Shares adding any school and mean net change in list length; the comparisons are descriptive. Source: `codeAnalysis/warning_placement_evidence.py`.

arm without a list gives the closest measure of the warning’s own effect, and the list’s contribution is identified by the randomized comparison of T_2 with T_1 (Section 6.4). The comparison group is drawn from the covered grades, where 1,221 of the 1,959 positive-risk applicants outside the experimental sample applied. Of these, 142 were nonetheless sent a report card (299 across all grades); we exclude them using the delivery roster, leaving 1,079 applicants, and report results under four definitions of the comparison group. The contacted group comprises the 1,608 experimental families whose recomputed baseline risk is positive (100 of the 1,708 had positive risk on the ministry’s send-time score but zero on the recomputed baseline) and the 142 non-experimental recipients, 1,750 in all.

Table 5 presents the effect of the 2021 outreach on realized non-placement: relative to never-contacted applicants with positive risk on the ministry’s send-time score, non-placement falls by 9.5 percentage points from a base of 38.2 percent (s.e. 2.9), significant at the 1% level. Restricting the treated group to the arm without a list (T_1 , which still received the warning, the report card and the re-

opened platform) yields an estimate of 8.3 points (s.e. 3.4), significant at the 5% level. The same comparison based on predicted risk yields 7.5 points, indicating close agreement between the realized and simulated measures.

The broader comparison groups yield smaller estimates, 4.1 points against applicants who applied after the outreach window and 5.6 points against all never-contacted applicants, a difference that reflects timing and reachability. Contact required a nine-digit guardian cell number, and among never-contacted applicants, having a working number is itself mildly predictive of non-placement conditional on baseline risk. Restricting the comparison to applicants who were both at risk and reachable, the population the ministry could in fact have contacted, yields 10.9 points (s.e. 3.0); we use the more conservative 9.5-point estimate in what follows. The delivery files do not record why these 237 reachable applicants were never sent a report card, and we cannot account for the omission; the coverage pattern and the observable balance described below are the only evidence that it was unrelated to placement prospects.

Contacted families have higher baseline risk than any comparison group (0.51 against 0.43 in the headline comparison), so Panels C and D examine how sensitive the estimates are to the adjustment for baseline risk. In the headline comparison, the unadjusted reduction is 3.2 points, while every adjustment yields estimates between 9.0 and 11.1 points. Against all never-contacted applicants, a group with substantially lower baseline risk, the unadjusted difference has the opposite sign (9.4 points), and every adjustment restores a reduction of 5.5 to 6.9 points. The estimates therefore depend on adjusting for baseline risk, but not on how that adjustment is specified. They identify the effect of the outreach package only if, conditional on baseline risk and grade, contact was unrelated to the placement outcome a family would have had without it; this is a selection-on-observables assumption that the data cannot test for the 237 reachable applicants left out. On what we observe, contacted applicants and the 267-family comparison group have the same grade mix (63 percent in grade 1 in both) and similar reachability (93 against 89 percent with a nine-digit number); district and initial list length are not available for applicants outside the experiment.

Panel B of Table 5 shows where the effect is concentrated. It is indistinguishable from zero in the lowest quartile of baseline risk, where 6.1 percent of the

Table 5: Effect of the 2021 Outreach on Realized Non-Placement

	Comparison group			Effect of outreach	
	<i>N</i>	Baseline risk	Unplaced	pp	(s.e.)
<i>A. By definition of the never-contacted comparison group</i>					
All never-contacted	1079	0.34	0.257	−5.6***	(1.7)
Applied after the outreach window	516	0.43	0.320	−4.1*	(2.1)
Positive send-time risk	267	0.43	0.382	−9.5***	(2.9)
Positive send-time risk, reachable	237	0.43	0.397	−10.9***	(3.0)
<i>B. By quartile of baseline risk, against all never-contacted</i>					
First quartile	607	0.14	0.061	−0.1	(1.9)
Second quartile	187	0.48	0.380	−4.3	(4.2)
Third quartile	146	0.61	0.486	−6.4	(4.7)
Fourth quartile	139	0.73	0.705	−16.5***	(4.6)
<i>C. Sensitivity to the baseline-risk adjustment, comparison: positive send-time risk</i>					
No adjustment (difference in means)				−3.2	(3.2)
Linear in baseline risk, grade				−10.0***	(2.9)
Quadratic in baseline risk, grade				−10.3***	(2.9)
Cubic in baseline risk, grade (Panel A)				−9.5***	(2.9)
Baseline-risk decile strata, grade				−9.0***	(2.9)
Rewighted to the contacted risk distribution (IPW)				−11.1***	(3.5)
<i>D. Sensitivity to the baseline-risk adjustment, comparison: all never-contacted</i>					
No adjustment (difference in means)				9.4***	(1.8)
Linear in baseline risk, grade				−5.6***	(1.7)
Quadratic in baseline risk, grade				−5.6***	(1.8)
Cubic in baseline risk, grade (Panel A)				−5.6***	(1.7)
Baseline-risk decile strata, grade				−5.5***	(1.8)
Rewighted to the contacted risk distribution (IPW)				−6.9***	(2.2)
Contacted families	1750	0.51	0.350		

Notes: The outcome is receiving no first-round placement, from the ministry’s post-assignment file. The sample is applicants with positive predicted baseline risk in the covered grades (PPK and grade 1). Treated applicants were sent a report card; comparison applicants were never contacted (they are absent from the delivery roster) and could not revise their lists. Panel A uses four comparison groups: all never-contacted applicants; those who applied after the outreach window, and so have no send-time score; those with positive risk on the ministry’s send-time score; and the latter restricted to reachable applicants, with a nine-digit guardian cell number on file (only this indicator is used). Estimates come from a linear regression of the outcome on a treatment indicator, a cubic in baseline risk and a grade dummy (linear baseline risk within quartiles), with HC1 standard errors. Panel B cuts quartiles on the contacted families’ baseline risk; its *N*, baseline risk and non-placement describe the never-contacted applicants in each quartile, and each estimate controls for baseline risk linearly within the quartile. Panels C and D re-estimate the third and first rows of Panel A, respectively, with no controls; a linear, quadratic or cubic polynomial in baseline risk plus the grade dummy; baseline-risk decile fixed effects plus grade; and a difference in means reweighted by inverse-probability weights from a logit of contact on the cubic and grade, which matches the comparison group to the contacted families’ risk distribution. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

comparison group went unplaced, and reaches 16.5 points in the top quartile (s.e. 4.6), significant at the 1% level, where 70.5 percent did. This pattern is consistent with the gradient predicted by P2, observed here on realized placement; panel (a)

of Figure 9 shows the same gradient for predicted risk. The quartile estimates of Panel B are measured against the broadest and most diluted comparison group; applied to the contacted population, they imply roughly 116 additional first-round placements from 1,750 messages, or 6.6 per hundred sent, whereas the 9.5-point estimate quoted above would imply about 166. We regard the smaller figure as the more conservative estimate. The distribution of that gain makes the practical case for a threshold rule: 17.5 percent of the messages went to families whose baseline risk was below 0.30, and only 9.4 percent of those families went unplaced. The 0.30 cutoff Peru adopted the following year, taken from Chile, would have excluded these messages; Section 7 discusses why the choice involves more than delivery cost.

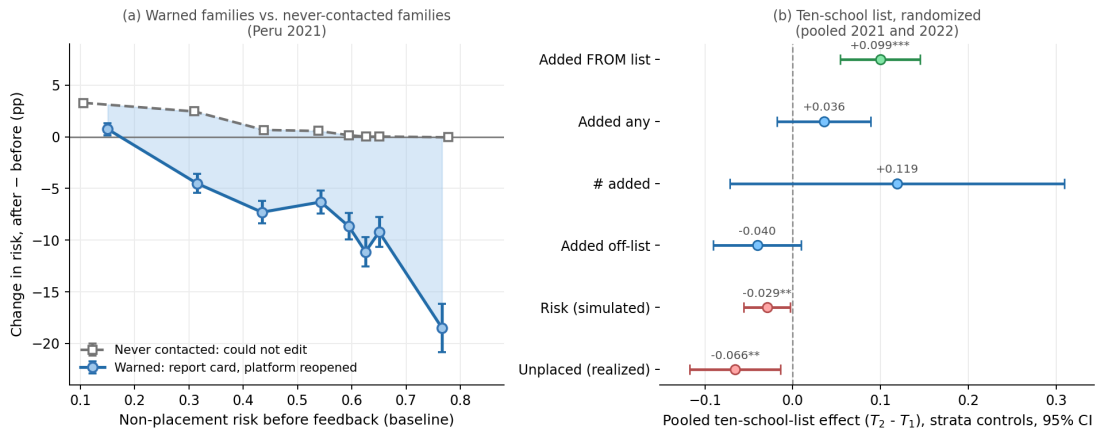
These are gains for the families who received the warning, and with a fixed number of seats a gain for one family can come at another’s expense. Panel A of Table 4 measures the net effect. Simulating the 2021 first round with the experimental families’ lists before and after the report card, and holding every other list fixed (only contacted families could revise), the expected number of unplaced children falls from 1,672 to 1,561: 110 more children placed, or 6.6% fewer unplaced. The warned families’ expected placements rise by 141 and other applicants lose 30, so about four-fifths of the warned families’ gain is net new placement rather than displacement. The arm that received the warning without a list gains nearly as much (7.6 fewer unplaced per 100 families) as the two list arms (8.7 and 8.4).

Panel (a) of Figure 9 plots the change in predicted risk against its baseline for warned applicants, who were contacted with the report card, and for positive-risk applicants in the same grades who were never contacted. Because in 2021 the platform was reopened only for the contacted, the second group could not revise at all. For both groups, baseline risk is recomputed by simulating deferred acceptance on the round-1 applications with the experimental families’ lists returned to their pre-intervention versions, so it is defined in the same way for the 516 never-contacted applicants who applied after the outreach window and have no send-time score. Octiles are cut on the contacted sample, so the two series are compared within the same bins of baseline risk. Among contacted applicants, risk falls further the higher the baseline, from -4.5pp in the second octile to -18.5pp in

the top (not monotonically), a proportional reduction of 12 to 24% across the upper seven octiles. This is the direction implied by P2 and by the compounding in equation (2), whose wedge is proportional to true risk.

The never-contacted series rules out a floor artifact: if the decline were mechanical, that series would fall too, but it never moves more than 3.3pp and stays within 0.2pp of zero above the median. The gap between the two series therefore widens by 19pp per unit of baseline risk (correlation 0.85 across octiles), the direction P2 predicts; Panel B of Table 5 recovers it on realized non-placement. The groups differ by selection into contact rather than randomization, and only contacted families could revise, so the comparison shows what happened when families received the information and could act on it.

Figure 9: Warned Families and the Suggestion List in Peru



Notes: Panel (a) compares warned families in Peru 2021, who were sent the report card and could revise because the platform was reopened for them, with at-risk families who were never contacted and could not revise. Contact was not randomized, so the panel is descriptive. It covers applicants with positive baseline risk in the grades the outreach covered. Each point is the mean change in predicted non-placement risk (after minus before, in percentage points) within an octile of baseline risk, plotted at the group's mean baseline risk within that octile; error bars are ± 1 standard error. Contacted applicants were sent a report card and had the platform reopened ($n = 1,750$); never-contacted applicants ($n = 1,079$; defined in Section 6.3) could not revise, so their movement reflects other families' edits. Octiles are cut on the contacted sample and applied to both groups; the shaded band is the gap between the two series. Panel (b) shows the randomized ten-school list: the pooled T_2 estimates of Table 9 among report-card openers in Peru 2021 and 2022, with 95% confidence intervals; the intervals for net schools added and realized non-placement extend beyond the plotted range. The risk estimates in panel (b) average over baseline risk; the data do not establish whether the list's effect varies with it (Section 6.5).

Non-contact within the positive-risk population mostly reflects timing and the contact information available to the campaign. The delivery roster was assem-

bled from the contact details held for each family, and applicants without usable details could not be sent anything; consistent with this, none of them appears in the delivery log under any status, so no message was attempted rather than attempted and refused. The comparison does not require contact to be random, only unrelated, conditional on baseline risk and grade, to the placement a family would have had without it. The data are consistent with this but cannot establish it: coverage ranges between 85 and 95 percent in every send-time risk band above 0.1, with no threshold and only a slight upward drift that is not statistically distinguishable from flat, and contacted and never-contacted at-risk applicants have the same grade mix, but these checks do not speak to selection on unobservables. Timing and reachability matter for the broadest comparison group, however: 44 percent of it lacks a usable number in the registration record and 48 percent applied late (the two overlap heavily, and only 46 percent of the group is neither), which is why it is the most diluted of the four.

We also examine whether the outreach package can be decomposed by delivery channel. Contact occurred through WhatsApp and email, while the platform was reopened for all contacted applicants. Some registered phone numbers were not linked to WhatsApp accounts, creating variation in whether the WhatsApp message could be delivered.²⁵ Combining this variation with report-card opening yields the four groups in Table 6.

The comparison is nevertheless uninformative. The relevant group, families whose WhatsApp message bounced and who did not open the report card, contains only 50 applicants. Its estimated effect is -12.8 percentage points (s.e. 7.2), but its raw non-placement rate differs from the comparison group by only two points, 0.360 versus 0.382; the larger adjusted estimate is driven by the baseline-risk specification. The adjacent group, whose WhatsApp message bounced but who opened the card, has only 31 applicants and an estimate of $+2.6$ points (s.e. 8.7). Every contacted applicant also received the report-card link by email, for which

²⁵On every failure the delivery log returns the same error, that the messaging provider cannot find the recipient on that channel, which identifies a number that is valid but not a WhatsApp account. It affects about five percent of contacted families. Email partly substituted for them, exactly as it did in Ecuador: 38 percent of these families still opened the report card, against 65 percent of those whose message went through; the Ecuadorian analogue in Table 2 is 36 percent without a delivered WhatsApp link against 76 percent with one.

receipt cannot be observed. The channels therefore cannot be separated in these data. The arm without a list (8.3 points, s.e. 3.4) and the threshold evidence above are therefore the best guides to the warning’s own contribution, and the randomized contrasts in Table 9 isolate what the list adds.

Table 6: The Outreach Package by Delivery Channel and Opening

WhatsApp send	Report card	N	Unplaced	pp	(s.e.)
Bounced	Not opened	50	0.360	-12.8^*	(7.2)
Bounced	Opened	31	0.452	$+2.6$	(8.7)
Delivered	Not opened	498	0.373	-7.5^{**}	(3.5)
Delivered	Opened	921	0.337	-12.1^{***}	(3.1)

Notes: Contacted applicants in the covered grades with positive baseline risk, split by whether the WhatsApp send completed and whether the report card was opened, each cell estimated against the same comparison group used in Table 5: never-contacted applicants carrying positive send-time risk ($N = 267$). “Bounced” means every send to that number failed with the provider’s channel-address error, which identifies a valid number that is not a WhatsApp account; the number itself is used only to match the delivery log. Outcome, sample restrictions, controls and standard errors are as in Table 5. No cell is powered to separate the channels: the bounced-and-not-opened cell holds fifty families, of whom five edited their portfolios. The four cells cover 1,500 of the 1,750 contacted applicants. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6.4 Recommendations: The Exploration Set Out in the Pre-Analysis Plan

The randomized arms explore what, if anything, recommendations add to the warning. The survey evidence in Section 5 motivates them, since families in both countries knew few of the schools near their home, but it also cuts both ways: many families believe adding a school hurts their chances at already-ranked options, and many draw no quality inference from popularity, potentially blunting the extra information in T_{3-2021} .

The list contrasts use the applicants who could be shown a list: those with positive predicted risk in Peru 2021 and those above the 0.30 threshold in Peru 2022 (applicants below it were also randomized but received a card without a list).²⁶ We report the list’s effect on all assigned applicants, which we lead with,

²⁶Predicted risk > 0 in Peru 2021 and > 0.3 in Peru 2022, the population shown the warning

and among families who opened the report card; Section 4.2 defines the two estimands and explains when the opener comparison is informative. Because the Ecuadorian T_{3-2021} arm opened the card more often than T_1 , we rely on the intention-to-treat estimate for that contrast. Table 7 reports intention-to-treat estimates for every contrast, and Appendix Table A9 rescales them by the T_2 opening rate.

Table 7 reports the list’s effects in each wave for all assigned applicants, the estimates we lead with, and Table 8 reports the same contrasts among families who opened the report card. Columns 1, 4 and 7 describe how T_1 families who opened the card revised their lists: in Peru 2021, 34% added a school, a net 0.9 schools per opener; in Peru 2022, 13%, a net 0.3. Section 6.2 uses these T_1 revisions as evidence on the warning; here they are the baseline for the list contrasts. The remaining columns report the differential effects of T_2 (the ten-school list) and the year-specific T_3 arms relative to T_1 .

Columns 1 to 3 show the effect in Ecuador. Ecuador’s lists excluded the programs (school by shift) a family had ranked rather than the schools, so 40% of them name a school already on the family’s initial application; the Ecuadorian rows for placement and enrollment at a listed school therefore count only listed schools that were not on the initial application, which the Peruvian lists exclude by construction. The first column indicates that 10% of applicants who opened a report card added at least one school to their list (15% of those whose card carried the warning and 5% of those whose card did not), and among those who did, list length rose by a net average of 2.5 schools. Columns 2 and 3 show no significant effect of providing school suggestions in Ecuador on the outcomes the list targets.²⁷ The ten-school list changed the share of applicants adding a listed school by 1.2 percentage points among all assigned applicants (s.e. 1.1) and by 0.5

and the list. In Ecuador 2021 the experiment randomized applicants regardless of predicted risk, and the report card carried the warning only for the 994 applicants (49%) whose simulated non-placement risk was at least 1% (Section 4). Because the risk bin is a randomization stratum, warning exposure is balanced across arms (331, 331 and 332 warned applicants), and the list contrasts remain valid.

²⁷Two coefficients on the T_{3-2021} arm are significantly negative: schools added (-0.112 , $p < 0.10$), and adding a school that was not on the list (-0.036 , $p < 0.05$). Given the number of contrasts estimated in this table, we do not read either as evidence of a behavioral response. The intention-to-treat estimates for this arm, on which we rely, are -0.033 (s.e. 0.030) and -0.012 (s.e. 0.008).

Table 7: The Suggestion List by Wave: Effects on All Assigned Applicants

Context	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ecuador 2021			Peru 2021			Peru 2022		
Intervention	T_1 Card	T_2 Card + list (10)	T_{3-2021} Card + list (10) + info pop/cong (diff.)	T_1 Warning	T_2 Warning + list (10)	T_{3-2021} Warning + list (10) + info pop/cong (diff.)	T_1 Warning	T_2 Warning + list (10)	T_{3-2022} Warning + list (3)
	(base)	(diff.)		(base)	(diff.)		(base)	(diff.)	(diff.)
<i>A. Choice behavior</i>									
Add any school	0.047	0.012 (0.012)	0.001 (0.012)	0.318	0.009 (0.028)	0.028 (0.028)	0.087	-0.001 (0.021)	0.018 (0.021)
Net schools added	0.108	0.068 (0.052)	-0.033 (0.030)	0.813	0.003 (0.090)	0.063 (0.090)	0.209	0.028 (0.069)	0.056 (0.082)
Add popular	0.021	0.008 (0.008)	-0.007 (0.007)	0.239	0.016 (0.025)	0.003 (0.025)	0.079	-0.008 (0.019)	0.005 (0.020)
Add congested	0.040	0.008 (0.011)	-0.004 (0.010)	0.297	-0.007 (0.027)	0.014 (0.027)	0.085	-0.003 (0.020)	0.012 (0.021)
<i>B. Add schools from list</i>									
Add from list (10)	0.039	0.012 (0.011)	0.003 (0.011)	0.180	0.063*** (0.024)	0.074*** (0.024)	0.024	0.016 (0.013)	
Add from list (3)							0.005		0.024** (0.009)
Add outside list (10)	0.030	0.000 (0.009)	-0.012 (0.008)	0.273	-0.048* (0.026)	-0.035 (0.026)	0.082	-0.011 (0.019)	
Add popular from list (10)	0.013	0.005 (0.007)	-0.003 (0.006)	0.054	0.025* (0.015)	0.020 (0.014)	0.013	0.013 (0.010)	
Add congested from list (10)	0.030	0.000 (0.009)	-0.001 (0.009)	0.143	0.039* (0.022)	0.050** (0.022)	0.021	0.013 (0.012)	
<i>C. Assignment and Enrollment Outcomes</i>									
Placement risk	0.249	0.003 (0.008)	0.005 (0.008)	0.398	-0.019 (0.013)	-0.016 (0.013)	0.351	-0.003 (0.010)	-0.014 (0.011)
Unplaced in round 1 (realized)	0.221	0.002 (0.014)	0.004 (0.014)	0.355	-0.046* (0.026)	-0.024 (0.026)	0.262	0.021 (0.029)	-0.006 (0.029)
Placed in list (10)	0.012	0.000 (0.006)	0.001 (0.006)	0.051	0.014 (0.014)	0.011 (0.014)	0.003	0.011* (0.006)	0.008 (0.006)
Enrolled in list (10)	0.111	-0.017 (0.015)	-0.017 (0.015)	0.141	0.009 (0.021)	0.002 (0.020)			
Observations	2,021	2,021	2,021	1,708	1,708	1,708	1,140	1,140	1,140

Notes. Columns 1, 4 and 7 describe applicants assigned to T_1 (report card without a list): the before-and-after change in the portfolio in Panels A and B, and the level of each assignment and enrollment outcome in Panel C. They are a descriptive benchmark rather than a causal effect; the other columns report differences from T_1 . T_2 adds the ten-school list, “list (10)” (columns 2, 5 and 8); T_{3-2021} adds popularity and congestion information for each listed school, “info pop/cong” (columns 3 and 6); T_{3-2022} replaces the ten-school list with a three-school list, “list (3)” (column 9). The sample is all applicants assigned to treatment. In Ecuador 2021 (“Card”) the report card carried the warning only for the 994 of 2,021 assigned applicants (49%) whose simulated non-placement risk was at least 1%; the other 1,027 had simulated risk below 1% (932 of them exactly zero) and received the card without it. The Observations row applies to Panels A and C. In Panel B, the Peru 2022 columns use 757 observations for the ten-school-list outcomes and 760 for “Add from list (3)”, because each list outcome is defined only for the arms shown that list; “Enrolled in list (10)” is not reported for Peru 2022, where enrollment is not observed. In Ecuador, whose lists excluded ranked programs (school by shift) rather than schools, “Placed in list (10)” and “Enrolled in list (10)” count only listed schools not on the initial application (`codeAnalysis/ecuador_list_outcomes.py`). Linear regressions on indicators for T_2 and T_3 , estimated separately for each wave, with controls for grade, district, placement-risk bin and gender (Appendix E.1); heteroskedasticity-robust (HC1) standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: The Suggestion List by Wave: Effects Among Report-Card Openers

Context	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ecuador 2021			Peru 2021			Peru 2022		
Intervention	T_1 Card	T_2 Card + list (10)	T_{3-2021} Card + list (10) + info pop/cong (diff.)	T_1 Warning	T_2 Warning + list (10) (diff.)	T_{3-2021} Warning + list (10) + info pop/cong (diff.)	T_1 Warning	T_2 Warning + list (10) (diff.)	T_{3-2022} Warning + list (3) (diff.)
	(base)	(diff.)	(diff.)	(base)	(diff.)	(diff.)	(base)	(diff.)	(diff.)
<i>A. Choice behavior</i>									
Add any school	0.097 (0.025)	0.005 (0.025)	-0.011 (0.023)	0.337	0.048 (0.036)	0.034 (0.036)	0.134	0.011 (0.038)	0.066* (0.038)
Net schools added	0.242 (0.117)	0.109 (0.117)	-0.112* (0.062)	0.896	0.123 (0.126)	0.004 (0.118)	0.330	0.119 (0.144)	0.163 (0.171)
Add popular	0.052 (0.019)	-0.001 (0.019)	-0.023 (0.016)	0.253	0.057* (0.034)	-0.020 (0.033)	0.121	-0.008 (0.035)	0.037 (0.036)
Add congested	0.086 (0.023)	-0.006 (0.023)	-0.021 (0.022)	0.311	0.034 (0.035)	0.014 (0.035)	0.134	0.004 (0.037)	0.049 (0.038)
<i>B. Add schools from list</i>									
Add from list (10)	0.082 (0.024)	0.005 (0.024)	-0.009 (0.022)	0.186	0.126*** (0.032)	0.095*** (0.032)	0.039	0.047* (0.026)	
Add from list (3)							0.011		0.050*** (0.019)
Add outside list (10)	0.064 (0.020)	-0.012 (0.020)	-0.036** (0.017)	0.303	-0.050 (0.034)	-0.069** (0.033)	0.127	-0.022 (0.035)	
Add popular from list (10)	0.033 (0.016)	0.005 (0.016)	-0.011 (0.014)	0.071	0.031 (0.021)	0.005 (0.020)	0.018	0.038* (0.020)	
Add congested from list (10)	0.071 (0.020)	-0.022 (0.020)	-0.021 (0.020)	0.145	0.085*** (0.029)	0.070** (0.029)	0.034	0.039 (0.024)	
<i>C. Assignment and Enrollment Outcomes</i>									
Placement risk	0.235 (0.013)	0.016 (0.013)	0.018 (0.013)	0.388	-0.039** (0.017)	-0.011 (0.017)	0.354	-0.017 (0.018)	-0.035** (0.018)
Placed in list (10)	0.030 (0.014)	-0.006 (0.014)	-0.005 (0.013)	0.055	0.030 (0.019)	0.010 (0.018)	0.006	0.018 (0.013)	0.016 (0.012)
Enrolled in list (10)	0.137 (0.025)	-0.040 (0.025)	-0.043* (0.024)	0.138	0.031 (0.027)	0.012 (0.027)			
Observations	879	879	879	1,076	1,076	1,076	525	525	525

Notes. As in Table 7, whose notes define the columns, the arms, the Ecuadorian “Card” and the list outcomes, for the applicants who opened the report card. For “Add from list (10)”, the Ecuador minus Peru 2021 difference in the T_2 effect is -0.121 (s.e. 0.040) among openers and -0.051 (s.e. 0.026) among all assigned applicants, computed from the two wave estimates, which come from independent samples. The Observations row applies to Panels A and C; in Panel B, the Peru 2022 columns use 345 observations for the ten-school-list outcomes and 362 for “Add from list (3)”. Linear regressions with strata controls and HC1 standard errors (Appendix E.1); Table 9 estimates each Peruvian contrast on the two arms it compares, so its estimates differ slightly. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

among openers (s.e. 2.4). Both are below the corresponding Peru 2021 estimates, by 5.1 percentage points among all assigned applicants (s.e. 2.6, significant at the 10% level) and by 12.1 among openers (s.e. 4.0, significant at the 1% level); each difference is computed from the two wave estimates, which come from independent samples. Because the shortfall holds among families who opened the report card, and among openers whose card carried the warning (-0.106 , s.e. 0.053), low reach does not explain it; Section 7 discusses the differences between the settings that coincide with it.

Columns 4 to 6 present results for Peru 2021. Among all assigned applicants, the ten-school list raised the share who added a listed school by 6.3 percentage points (s.e. 2.4) and the T_{3-2021} list by 7.4 (s.e. 2.4), from a base of 18%, both significant at the 1% level (Table 7). Among families who opened the report card, the effects are 12.6 and 9.5 percentage points (columns 5 and 6), increases of 68 and 51% over the T_1 base. The list changed which schools families added more than how many: its effects on adding any school and on net schools added are small and not significant, both among openers and among all assigned applicants.

Turning to the types of schools added, the differential effect of T_{3-2021} relative to T_2 is small and mostly indistinguishable from zero in Peru 2021. Families also shown popularity and congestion information added fewer popular schools (-0.076 , s.e. 0.033), but we treat this as suggestive: 2 of the 29 differential contrasts in Appendix Table A7 are significant at the 5% level.²⁸ Section 6.5 returns to this arm, because it ends up with a weaker effect on placement.

For Peru 2022 (columns 8 and 9), among all assigned applicants the ten-school list raised the share adding a listed school by an insignificant 1.6 percentage points (s.e. 1.3) and the three-school list by 2.4 (s.e. 0.9) (Table 7); among openers the estimates are 4.7 points (s.e. 2.6), 120% of the T_1 base, and 5.0 points (s.e. 1.9). Neither list significantly changed the number of schools added. The two list lengths had similar incremental effects on adding a listed school, consistent with P4, although the test is weak: 4.7 points for the ten-school list against a T_1 base of 3.9%, and 5.0 for the three-school list against a base of 1.1%, a difference in

²⁸Appendix Table A7 presents estimates for the differential effect of T_3 versus T_2 . The other significant contrast is in Ecuador, where the same arm added fewer schools on net (-0.225 , s.e. 0.112), which we treat as suggestive for the same reason; none of the remaining differentials is significant at the 10% level.

effects of 0.3 points (s.e. 3.2); the level comparison in Appendix Table A7 (-2.7 , s.e. 2.8) mixes these different baselines. Because the 2022 T_1 card did not carry the warning, both 2022 contrasts with T_1 combine the list with the warning’s appearance on the card; the T_{3-2022} increase in adding any school (7 percentage points, $p < 0.10$) may reflect that.²⁹

Appendix Table A2 pools the $T_2 - T_1$ contrast across waves. Across the two Peruvian waves (columns 3 and 4), the list raised the share of openers adding a listed school by 9.9 percentage points (s.e. 2.3) and lowered simulated non-placement risk by 3.0 points (s.e. 1.3), about 8% of the T_1 level (2.9 in the two-arm specification of Table 9, which we use in the text), and the share placed at a listed school rose from 4 to 7% (significant at the 10% level). Columns 1 and 2 add Ecuador, whose estimates are close to zero, so we do not use them for inference about any one setting.

The reallocation toward suggested schools is concentrated in popular options. Across the pooled sample, families added 1.9% of suggested schools without the list and 3.5% when the list was shown; in Peru, the corresponding rates were 1.9% and 4.2%. Popular schools were added 4.4% of the time without the list and saw the largest response to being suggested, increasing by 3.4 percentage points in the pooled sample and 5.0 points in Peru. Less popular suggestions generated smaller responses, although the effect remained significant in Peru. Appendix Table A5 reports the underlying school-level estimates.

6.5 Recommendations Pooled Across the Peruvian Cohorts

We pool the two Peruvian cohorts, comparing the ten-school suggestion-list arm (T_2) with the baseline arm (T_1), and absorb their different populations with a cohort fixed effect. Two caveats apply. The first concerns the warning: in 2021 it appeared in the report card for every arm, so the contrast isolates the list, whereas in 2022 the baseline arm received the warning only by WhatsApp, so the contrast combines the list with the warning’s appearance in the report card. The

²⁹The report card included a warning in T_2 and T_{3-2022} but not in T_1 ; all three arms received a separate WhatsApp warning. See Table A1 in Appendix B for details on each intervention.

second concerns the third arm, which is not the same intervention in the two years: in 2021 T_3 added popularity and congestion information to the same ten-school list, while in 2022 it replaced the list with a three-school version. We therefore report it by cohort rather than pooling it with T_2 . Table 9 reports these effects among warned applicants who opened the report card, with saturated controls for the randomization strata. Because each contrast is estimated on the two arms it compares, the estimates differ slightly from those in Table 8, which estimates all three arms of a wave jointly.

Three patterns emerge. First, report cards containing suggestions redirect applications toward the named schools. The probability of adding a listed school rises by 0.099 in the pooled sample, significant at the 1% level, with positive estimates in every arm and cohort. Off-list additions decline, but the estimates for T_2 are not statistically significant (-0.040 pooled). Neither the probability of adding any school nor the net number of schools added increases significantly, with estimates of 0.036 and 0.119, respectively. The response therefore concerns which schools families add more than how many they add, consistent with P3.

Second, among openers the ten-school list lowered simulated non-placement risk by 2.9 percentage points in the pooled Peruvian sample (s.e. 1.3) and by 4.0 in Peru 2021 (s.e. 1.7), both significant at the 5% level; in Peru 2022 the ten-school estimate is an insignificant 1.6 points, and the three-school list lowered risk by 3.7 points (s.e. 1.8). Simulated risk averages over many draws of the deferred-acceptance lottery, while realized first-round non-placement is the outcome of the single lottery the ministry ran, so the realized measure tracks the same probability with more noise; across bins of simulated risk the two are closely aligned (correlation 0.988). The realized estimates are larger, 6.6 percentage points pooled (s.e. 2.6) and 9.4 in Peru 2021 (s.e. 3.3), but only one of the four arm-by-wave cells in Table 9 is significant, the pooled estimate falls to 2.6 points (s.e. 4.0) without the largest district,³⁰ and for all assigned applicants the estimates are -4.6 points in

³⁰The experiment was concentrated in one district, so we re-estimated the effect leaving out one district at a time. Dropping the largest district (632 of 1,052 openers) leaves -0.026 (s.e. 0.040), while dropping any other district leaves estimates between -0.063 and -0.084 , all significant at the 5% level. The sensitivity comes from the second 2022 district rather than from Peru 2021. Within Peru 2021 the estimate is -0.094 (s.e. 0.033) overall and -0.078 (s.e. 0.047) without the largest district, where it loses conventional significance because the sample falls from 707 to 298; within Peru 2022 it is -0.013 (s.e. 0.044) overall but $+0.099$ (s.e. 0.066) on

Peru 2021 (s.e. 2.6, significant at the 10% level) and +2.1 in Peru 2022 (s.e. 2.9) (Table 7).

Finally, the evidence does not establish whether the incremental effect varies with baseline risk. The pooled interaction of T_2 with baseline risk on realized non-placement is -0.152 (s.e. 0.099). Its direction is consistent with larger reductions among higher-risk families, but the estimate is not statistically distinguishable from zero.

These estimates condition on opening the report card after randomization and therefore describe families who engaged, not the average assigned family. For all assigned applicants, the ten-school list’s effect on simulated risk is -1.9 percentage points in Peru 2021 (s.e. 1.3) and -0.3 in Peru 2022 (s.e. 1.0), neither significant (Table 7). Opening rates differ in two arms: the Ecuadorian T_{3-2021} arm opened the card 9 percentage points more often than T_1 , and the Peruvian T_{3-2021} arm 4.9 points more often ($p < 0.10$); the T_2 gap in Peru 2021, 4.0 points (s.e. 2.9), is not significant. These patterns are why we lead with intention-to-treat estimates (Section 4.2).

Baseline balance provides a second qualification. Among Peru 2021 openers, T_2 starts with slightly lower risk than T_1 (-0.032 , significant at the 10% level), but the gap falls to an insignificant -0.011 after accounting for the randomization strata included in Table 9. Controlling for baseline risk barely changes the realized non-placement estimate (-0.091 versus -0.094 , both significant at the 1% level). Appendix Tables A3 and A4 report balance for all assigned applicants and openers. No baseline covariate differs at the 5% level in either sample, and no joint test rejects balance. The initial-risk gap among Peru 2021 openers is the closest to the 5% threshold.

The randomized contrasts show that report cards containing suggestions redirect applications toward the named schools, with a reduction in simulated non-placement risk concentrated in Peru 2021 (panel (b) of Figure 9). In Peru 2022, this contrast combines recommendations with additional risk feedback on the card. The three-school list also redirects applications in that wave, consistent with a

the 122 openers in the remaining district, and the pooled estimate without the largest district averages the two. Dropping the largest district from Peru 2021 alone leaves the pooled estimate at -0.041 (s.e. 0.032).

Table 9: Effect of the Suggestion List, by Arm and Cohort

Effect relative to baseline arm (T_1)	Peru 2021		Peru 2022		Pooled
	T_2	T_{3-2021}	T_2	T_{3-2022}	T_2
Added a school from the list	0.126*** (0.032)	0.094*** (0.032)	0.047* (0.026)	0.050*** (0.019)	0.099*** (0.023)
Added any school	0.048 (0.036)	0.033 (0.036)	0.009 (0.038)	0.069* (0.038)	0.036 (0.027)
Net schools added	0.128 (0.127)	0.002 (0.117)	0.106 (0.144)	0.171 (0.174)	0.119 (0.097)
Added a school not on the list	-0.049 (0.034)	-0.069** (0.033)	-0.022 (0.035)	0.038 (0.036)	-0.040 (0.026)
Non-placement risk (simulated)	-0.040** (0.017)	-0.010 (0.017)	-0.016 (0.018)	-0.037** (0.018)	-0.029** (0.013)
Unplaced in round 1 (realized)	-0.094*** (0.033)	-0.022 (0.034)	-0.013 (0.044)	-0.018 (0.043)	-0.066** (0.026)
Observations	707	710	345	362	1052
Strata controls	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	—	—	—	—	Yes

Notes: Effects relative to the baseline arm (T_1) among warned applicants who opened the report card. Each estimate comes from a linear regression on the two arms it compares, with saturated controls for the randomization strata (grade, district, risk bin, gender); the pooled column adds a cohort fixed effect and reports T_2 only, because the third arm differs between years. Simulated non-placement risk is the deferred-acceptance probability; realized non-placement indicates no first-round placement. Within Peru 2021, equality of T_2 and T_{3-2021} is rejected for realized non-placement ($p = 0.03$) and marginal for simulated risk ($p = 0.10$). HC1 standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

longer list not being necessary for a behavioral response.

One result departs from this pattern. In 2021, the third arm received the same ten-school list as T_2 , together with information on each school’s popularity and congestion. Yet its realized non-placement estimate was smaller: non-placement fell by 9.4 percentage points under T_2 , compared with an insignificant 2.2 points under T_{3-2021} , and equality of the two estimates is rejected at the 5% level. The arms were balanced at baseline and opened the report card at similar rates, so observed differences in composition do not explain the pattern.

The mechanism is uncertain. Relative to T_1 , families in T_{3-2021} were nearly as likely as T_2 families to add a listed school, did not add more schools on net, and made fewer off-list additions (-0.069 , s.e. 0.033); none of these differ significantly from T_2 (Appendix Table A7). The pattern is consistent with the additional information making families more selective rather than encouraging further search,

but the individual contrasts do not establish a single mechanism, so we treat this as a suggestive interpretation. The data do not show that popularity information improved on the list alone.

6.6 Why the Response Was Weaker in Ecuador

The Ecuadorian response was weaker on both margins. Among openers whose card carried the warning, 15% added a school, against 34% of warned openers in Peru 2021, and the recommendation arms had no detectable effect. We note two implementation details here; Section 7 compares the settings more fully.

First, the intervention in Ecuador did not include a separate WhatsApp warning message on placement risk; the warning appeared only within the report card, which may have reduced its salience,³¹ and only for the 49% of the sample whose simulated non-placement risk was at least 1%. Combined with implementation challenges that limited WhatsApp outreach to only 22% of families, the effective treatment intensity in Ecuador was substantially lower than in Peru. The recommendation arms' null is a different matter, however: their shortfall relative to Peru 2021 holds among families who opened the report card.

Second, platform access differed: in Peru 2021, applicants could only submit once and received special platform access through our intervention, while in Peru 2022 and in Ecuador all applicants could freely modify their lists. This procedural difference may partly explain the variation in point estimates across the two Peruvian waves.

6.7 Survey Results

We evaluated whether the additional suggestion lists (in T_2 , T_{3-2021} and T_{3-2022}) and the information on popularity and congestion (T_{3-2021}) impacted subjective measures captured by our survey of applicants. We find evidence related to the perception of the application process: applicants who received suggestion lists in 2021 Peru were less likely to say that it was hard to search for schools. In 2022

³¹There is an emerging literature on people's limited capacity to pay attention to all the potential attributes in the choice process, and efforts have been made to incorporate this into economic models. See [Gabaix \(2019\)](#) for a review.

Peru, students assigned to T_2 or T_{3-2022} were more likely to declare that they received the warning message. This is consistent with the implementation, since families in T_1 only received the warning through WhatsApp, and not in the report card.³²

Applicants in Ecuador assigned to T_2 rated the quality of the “Information about schools available on the application platform” lower than other groups. We interpret this result with caution. First, the treatment is not directly related to the information available on the platform. Second, the result does not hold for T_{3-2021} . Third, the Ecuadorian columns of Table A8 report 22 contrasts (11 outcomes for each of two arms), so a type I error is likely.

The treatments had no effect on the declared satisfaction with hypothetical placement results (Panel B). Our intervention did not aim to promote changes in the first preference, but we did expect to affect the lowest-ranked option since the invitation was to “add more schools to the list.” Applicants who received the suggestion lists did not declare a lower level of satisfaction with the schools chosen at the bottom of the rank-order list.

Of up to five schools drawn from each family’s suggestion list, Peruvian T_1 respondents in 2021 declared that they were aware of 36% (Table A8, Panel C). For those assigned to T_2 , the share is 14 percentage points higher (s.e. 5.6), equivalent to 0.7 more listed schools, but part of this gain is mechanical: the measure also counts listed schools that the family went on to add to its application. In Ecuador, T_2 respondents declare knowing fewer listed schools (−7.4 percentage points, s.e. 3.2), but they also report knowing their own applied schools less well, so the coefficient does not show that the list reduced awareness (Appendix H).

Section 7 asks what the contrast between Peru and Ecuador says about scaling.

7 Discussion: Scaling and Comparability Across Contexts

The comparison across Chile, Peru, and Ecuador separates two questions: whether the frictions that motivate smart matching platforms travel, and whether the

³²See Table A1 for details on the contents of each treatment.

information can be delivered so its benefits survive scale. The frictions are similar across settings, and the warning changed applications and placement in Peru as it did in Chile. The response was weaker in Ecuador, where the warning reached fewer families and appeared only on the report card, and the recommendation lists, the exploration set out in our pre-analysis plan, redirected applications in Peru but not in Ecuador.

This section asks whether the cross-site pattern is consistent with P5; with three implementations it cannot test it. The raw comparison confounds frictions that differ with common frictions the information failed to engage, and we take these in turn.

7.1 Consistent Frictions, and a Warning That Transports

In Peru and Ecuador, families knew, at least by name, between a fifth and a third of the nearby schools their report card would list (36%, 36% and 21% across the three waves), and applicants with positive non-placement risk overestimated their admission chances by 20 to 37 percentage points on average, depending on country and year (Figure 5). These frictions are comparable to the Chilean evidence (Arteaga et al., 2022). Figure 4 shows optimism well above zero in all three countries, although its level differs (Appendix Table A10), and Figure 6 shows that optimism and lack of options are common reasons families stop searching.

The warning transported to Peru. Families who received it revised their applications and families who did not barely did: in 2022, 9.3 percent of warned applicants added a school, against 1.7 percent of applicants below the threshold, who could revise but received a plain report card (Table 4), and the discontinuity at the threshold (Figure 7) has the same sign as the Chilean estimate, although it is imprecise in narrow windows. In 2021, warned families were less often unplaced in the first round than comparable families who were never contacted (Table 5), and the simulated first round places 110 more children. These comparisons are not randomized, and in 2021 only contacted families could revise, so the warning arrived together with the report card and the chance to act on it. In 2021, when every report card carried the warning, families who received it without a list re-

sponded nearly as much as those who also received one, while in 2022 the jump at the threshold appears in the arms whose card carried the warning and not in the arm warned by WhatsApp alone, a difference that is imprecise.

The Peruvian results therefore replicate, in a new system run by another government, the Chilean evidence that placement-risk feedback induces families to expand portfolios, add safer options, and reduce non-placement (Arteaga et al., 2022). Follow-up evidence from Chile further finds that these short-run changes are associated with sustained gains in academic achievement and school quality up to five years after placement (Arteaga et al., 2026).

7.2 Why Ecuador Responded Less: Delivery, Search and Context

Ecuador separates the presence of information frictions from a behavioral response. The frictions are present at comparable magnitude (Section 5), but the response to the warning was weaker: among openers whose card carried the warning, 15 percent added a school, against 34 percent of warned openers in Peru 2021, and the warning was delivered only on the report card, with no separate WhatsApp message. The randomized $T_2 - T_1$ contrast, which adds a suggestion list of up to ten schools, shifted Peruvian applications toward listed schools but produced no detectable response in Ecuador. In Peru 2022 that contrast combines recommendations with additional risk feedback on the report card, while both arms received a separate WhatsApp warning (Section 4).

Weaker delivery plausibly explains part of the weaker warning response. It does not explain the null of the recommendation arms, which holds among openers, in a wave where few families searched at all, and amid other differences that the design cannot separate.

First, delivery reach differed across contexts. In Peru, 89–92% of targeted families read the introductory WhatsApp message (Table 2). In Ecuador, WhatsApp outreach reached only 22% of the treatment group, and email provided partial backup, bringing total report-card views to 43%.

Ecuador’s shortfall arose at the coverage stage: the contact roster did not yield usable numbers for 78% of the sample, while messages actually dispatched were

delivered and read at rates comparable to Peru’s (97% and 90%). Peru’s coverage fell short in other ways. In 2021 the ministry permitted outreach to positive-risk applicants in the covered grades, yet 1,079 of them were never contacted: 516 applied after the outreach window, 296 had zero risk on the ministry’s send-time score, and 267 were at risk on the send-time score, 237 of them with a nine-digit cell number on file, yet were never sent a card, for no recorded reason (Section 6.3). In 2022 the warning was restricted to applicants above a 0.30 risk threshold. But the Ecuadorian null holds among the families the card reached. Among openers, the list’s effect on adding a listed school was 0.005 (s.e. 0.024), a difference from Peru 2021 of -0.121 (s.e. 0.040). Among openers whose card carried the warning, the group for which P3 predicts a list effect, it was 0.020 (s.e. 0.042), a difference of -0.106 (s.e. 0.053).

Second, Ecuadorian families searched little. In Peru 2021 the list worked mainly by redirecting search already under way (Section 6.5): it lowered by 0.078 (s.e. 0.024) the share of openers whose only additions were unlisted schools. Ecuador had little such search to redirect. Among T_1 openers, 9.7% added any school, against 33.7% in Peru 2021, and 1.5% added only unlisted schools, against 15.2%. Of the 26 Ecuadorian T_1 openers who added a school, about 85% added one their list would have named, against 55% in Peru 2021.

Other differences between the settings cannot be separated by the design. Ecuadorian families were choosing for the first time, having previously been assigned by location, which may have limited their ability to act strategically on new information. Ecuador’s platform stayed open to revisions throughout, as Peru’s did in 2022, when effects were also smaller; in Peru 2021, the wave with the clearest effect, it was reopened only for contacted families. Ecuador sent no separate warning message, and its card carried the warning only for the 994 applicants (49%) with simulated non-placement risk of at least 1%; the other 1,027 had risk below 1%, 932 of them exactly zero. Average placement risk was therefore lower in Ecuador, but not among applicants whose card carried the warning (a control-arm mean of 0.51 as printed on the card, against 0.48 recomputed from the initial applications of Peru 2021’s control arm). Opening the card also does not guarantee that the list was seen: Ecuador’s mobile mockup placed the list behind an expand button, unlike Peru 2021’s, which showed it inline, and we cannot verify

whether the live card did so or how many openers expanded it.

Market structure also differed. In the experimental samples, applicants had on average 13 on-platform and 14 off-platform options within 2 km in Ecuador, against 19 and 28 in Peru 2021 and 20 and 30 in Peru 2022 (Table 1; Figure 1). Ecuadorian applicants had fewer alternatives nearby, though not few: the median family had 12 schools with open seats within 3 km that were on neither its list nor its initial application. In terms of equation (4), fewer of them may have improved on the fallback. Density does not order the effects, however: Peru 2022, the densest market, had a ten-school intention-to-treat effect of 0.016 (s.e. 0.013), indistinguishable from Ecuador’s 0.012 (s.e. 0.011).

How the list works is less clear. The Peruvian response is consistent with the framework in Section 2: warned families who opened the card often revised their lists, and those shown a list disproportionately added listed schools. The design does not identify the channel.

The survey is suggestive but incomplete: T_1 families knew 36%, 36% and 21% of the schools that would have appeared on their list in Ecuador, Peru 2021 and Peru 2022 (Table A8, Panel C), so most of them were new, and in Peru 2021 the list raised declared awareness by a statistically significant 14 percentage points, although this measure also counts listed schools the family went on to apply to. But we cannot disentangle whether families added schools because the list informed them, made known options more salient, or created urgency that lowered the threshold for action. The data are consistent with multiple channels operating at once; isolating them would require randomizing list content independently of the warning.

Many families also misunderstand strategy-proofness: they believe that adding schools to their list will harm their chances at higher-ranked options (Figure A1). This belief works against portfolio expansion, yet warned Peruvian families who opened the card often revised their lists and, when shown a list, redirected applications toward suggested schools. The data cannot tell whether the intervention overcame this misperception or reached mainly families who did not hold it. An intervention that explains the strategy-proof mechanism directly would separate the two.

Two further points concern the estimates themselves. First, the intention-to-

treat and opener-conditional estimates are linked by the opening rate. Because no T_1 family receives a list, the difference in list receipt between arms equals the T_2 opening rate: 0.42 in Ecuador 2021 and 0.64 in Peru 2021 (Table 2, Panel E). Dividing the $T_2 - T_1$ intention-to-treat estimate by that rate gives the Wald estimate with list receipt as the treatment. For adding a school from the list it is about 0.10 in Peru 2021 and 0.03 in Ecuador 2021; Appendix Table A9 applies a similar adjustment to the other outcomes most directly affected by the list.

The rescaled estimate equals the effect among openers only if assignment does not affect families who did not open the card. In Ecuador those families show a $T_2 - T_1$ difference of 0.014 (s.e. 0.009) in adding a listed school, which is why the rescaled 0.03 exceeds the direct estimate among openers, 0.005 (s.e. 0.024). We therefore treat the rescaling as descriptive. Because opening occurs after randomization, both it and the opener-conditional estimates describe the families the card reached rather than the full applicant population.

Second, the Ecuadorian null is not a power artifact. At conventional significance and power, the minimum detectable intention-to-treat effect on adding a school from the list was 3.1 percentage points (Appendix F), roughly half the Peru 2021 effect of 6.3 points, and a 12.6-point effect among openers, the Peru 2021 estimate, would translate into a 5.3-point intention-to-treat effect at Ecuador’s 42% opening rate. At the intention-to-treat level, the difference between Ecuador and Peru 2021 is -0.051 (s.e. 0.026). The pattern is a failure to transport across sites of the kind documented by Allcott (2015) and Vivalt (2020): Ecuador was a contemporaneous second site, not a scale-up of the Peruvian pilot.

These calculations, like all opener-conditional estimates, are exploratory: the pre-analysis plan named outcomes rather than estimands and did not contemplate conditioning on opening. Our evidence on the recommendation arms therefore leads with the intention-to-treat estimates for all assigned applicants (Table 7): the list’s effect on adding a school it named is 0.063 (s.e. 0.024) in Peru 2021, significant at the 1% level. The pooled specification of Section 6, the Peru 2022 wave and the results on realized placement were not pre-specified either.

7.3 Implications for Scaling Smart Matching Platforms

The comparison suggests a practical diagnostic for adopting smart matching in a new market. Policymakers should assess how families already search, whether information reaches them through a salient channel early enough to be used, and whether it points toward a meaningful set of alternatives; they should also consider whether an existing platform can spread fixed costs across cohorts and regions. In our setting, more than 60% of families opened the report card in Peru 2021, compared with 43% in Ecuador, yet the null of Ecuador’s recommendation arms holds among the families the card reached, and in both countries the cards arrived 4–5 days before the deadline. Chile’s cost benchmarks of \$0.33 per student and \$0.52 per 0.1 standard deviation of test-score gain apply only where delivery, timing, and market conditions allow the information to change behavior ([Arteaga et al., 2026](#)).

Table 10 maps these conditions to measurable indicators and reports their values across Chile, Ecuador, and both Peruvian waves alongside the corresponding treatment effects. With four implementations, the table is descriptive, not a prediction model. The clearest list response came in Peru 2021, which combined a dense market, high reach and a platform reopened only for contacted families. Peru 2022, with a market at least as dense and an opening rate close to Ecuador’s (46% against 43%), showed a smaller response; Ecuador showed none we can detect. The indicators can be measured before a rollout, but three list implementations cannot say which of them matters.

The cost structure helps explain why these diagnostics matter. The largest cost, building the smart-matching platform and integrating it with the ministry’s assignment system, is fixed: it need not be incurred again in each admission cycle or in each region of the same country. Once the platform exists, the relevant cost is the marginal cost of reaching one more at-risk applicant, dominated by WhatsApp messaging at about \$0.03 to \$0.05 per at-risk student in our implementations. Because only applicants with meaningful non-placement risk are warned (about a quarter of applicants in each of our samples), the cost averaged over all applicants is lower still. This makes the intervention potentially attractive at scale, but only if the benefits also travel. In Ecuador marginal costs stayed low, but a list that does not change applications yields no benefit to set against them.

Table 10: Scaling Conditions and Outcomes Across Implementations

	Chile 2018–2020 national, all grades	Ecuador 2021 Manta, PPK–K	Peru 2021 Tacna, PPK and 1st	Peru 2022 Tacna, PPK–1st
<i>A. Delivery reach and salience</i>				
WhatsApp/platform message sent (%)	100 [†]	22	100	100
Report card / pop-up opened (%)	—	43	63	46
Separate warning message	Yes [†]	No	Yes	Yes
<i>B. Market structure</i>				
On-platform options within 2 km	—	11	16	17
Off-platform options within 2 km	—	12	19	20
Pre-intervention ROL length	2.77 [‡]	1.83	3.14	3.16
<i>C. Intervention parameters</i>				
Risk threshold for treatment	30%	None [§]	> 0	30%
Share of applicants treated (%)	20 [‡]	51	25	23
Days before deadline	—	4–5	4	4
<i>D. Outcomes</i>				
Risk-threshold RD: effect on placement risk	−0.033***	—	—	—
Risk-threshold RD: effect on schools added	0.340***	—	—	Positive, imprecise
List effect: add from list ($T_2 - T_1$)	—	0.005	0.126***	0.047*
List effect: add any school ($T_2 - T_1$)	—	0.005	0.048	0.011
Effect on placement risk ($T_2 - T_1$)	—	0.016	−0.039**	−0.017

Notes. Panel B draws on all applicants in Table 1 (columns 1, 4 and 7); among RCT participants (columns 2, 5 and 8) the on- and off-platform counts are 13 and 14 in Ecuador, 19 and 28 in Peru 2021, and 20 and 30 in Peru 2022. Panel D draws on Table 8, whose estimates are among report-card openers, and on Figure 7; for all assigned applicants the list’s effect on adding a listed school is 0.012, 0.063*** and 0.016 (Table 7). The Chilean column is from [Arteaga et al. \(2022\)](#). Treated shares are not comparable across columns (Section 4). At the 0.30 cutoff only the warning switches on in Chile, but the whole report card does in Peru 2022 (Section 6). In Peru 2021 the rule permitted outreach to every positive-risk applicant in the covered grades, but 1,079 of them were not contacted (Section 6.3). [†] The Chilean pop-up was shown at submission and counted only for applications flagged risky, so its reach is not comparable to the other columns. [‡] Closest Chilean construct: list length at first submission, and the share whose first attempt carried risk above 0.30 (attenuated because the pop-up ran only from the third day of the 2018 and 2019 windows). [§] Ecuador randomized applicants regardless of predicted risk; the report card carried the warning only for the 994 of 2,021 applicants (49%) whose simulated non-placement risk was at least 1%. “—” marks a quantity that is undefined or not comparable; in particular, Chile’s nearby-option counts use a 3 km radius without the on/off-platform split, and the Peru 2022 discontinuity in placement risk is not identified (Section 6). *** p<0.01, ** p<0.05, * p<0.1.

The budgetary accounting omits the institutional cost of communicating risk. As Section 6 shows, ministries did not treat a warning that a child might go unplaced as costless, even when the monetary expense of delivery was low. These political or reputational costs can rise with coverage. Informing a small group raises different concerns from warning a large share of an entering cohort, especially while a new assignment system is establishing legitimacy. Delivery reach therefore reflects not only technical capacity but also the sender’s perceived cost of communicating risk. Designs that frame feedback as routine guidance or incorporate it into report cards sent to all applicants may increase effective reach without changing the underlying technology.

These concerns may be particularly acute during initial implementation. By that point, the mechanism, platform, eligibility rules, and risk threshold are often fixed, while the content and breadth of outreach can still be adjusted. Institutional caution can therefore fall disproportionately on the information the intervention depends on, producing systematic attenuation in early rollouts or settings where officials face greater scrutiny. This suggests the hypothesis that estimates from an initial rollout understate the effect of the same design once the system is more established. The implication differs from the usual voltage-drop concern, in which pilot estimates overstate effects at scale (List, 2022, 2026). Here, the implementation environment may itself suppress treatment intensity.

Two further threats to scaling remain. Our analysis addresses several of the “vital signs” identified by List (2022), namely non-representative populations, implementation fidelity and statistical power; these two it does not.

First, equilibrium effects, which our design does not identify. The intended policy is to provide placement-risk feedback to all at-risk applicants. In Peru 2021 the outreach contacted 1,750 of the 2,829 applicants with positive baseline risk in the covered grades (about 62%; the denominator includes 516 who applied after the outreach window and 296 with zero risk on the ministry’s send-time score, and within send-time risk bands coverage was 85 to 95 percent, Section 6.3), and the closed platform kept the others from revising their lists; placement differences between the two groups are therefore relative. In Ecuador every applicant in the experimental sample was sent the report card, regardless of risk, and the card carried the warning for the 994 (49%) with simulated non-placement risk of at

least 1%. The randomization between T_1 , T_2 , and T_3 varied the intensity of information, namely whether families received a suggestion list on top of the baseline report card. The remaining question is what would happen under stronger effective treatment intensity, for instance if delivery reach were higher or behavioral responses larger. Such changes could shift congestion patterns across schools. Our results show that treated families add both popular and less popular schools from the curated list (Appendix Table A5), with the larger response at congested schools (Table A2), so the intervention may intensify congestion at some schools even as it fills seats at others; the net equilibrium effect remains an empirical question. Counterfactual simulations of the deferred acceptance mechanism could address it.

A related caveat concerns information spillovers: a warned family may pass the information to neighbors or other parents, so some treatment reaches families we classify as untreated and our randomized contrasts understate the full effect. The Peru 2022 design allows a test, which we leave to future work: risk feedback was withheld below the 0.30 threshold, so families below it in areas dense with treated applicants can be compared with otherwise similar families in areas with few.

Second, both Tacna and Manta were selected partly because local governments were supportive of reform, a form of “site selection” that may inflate observed effects relative to less cooperative environments (Allcott, 2015; List, 2022). Cooperation enabled the data sharing behind personalized risk calculations and the infrastructure for timely delivery; where administrative capacity or political will is lower, additional barriers may bind. Future implementations should monitor these conditions to see whether effects survive the move from pilot to policy.

8 Conclusion

Warning families about their placement risk changed their applications and their placement in Peru, as it did in Chile, while the response in Ecuador was weaker. The evidence supports three conclusions.

First, the frictions are general. Using linked surveys and administrative data from Peru and Ecuador, we document that the information frictions identified in

Chile’s school choice system ([Arteaga et al., 2022](#)) are not idiosyncratic to one institutional setting. Families in Peru and Ecuador knew, at least by name, between a fifth and a third of the nearby schools a suggestion list would name and overestimated placement probabilities by 20 to 37 percentage points, comparable to the Chilean evidence. Limited awareness of options and optimism about admission chances appear in all three systems, so the mechanism the warning targets is not specific to Chile.

Second, the warning result replicates. Families had already submitted their applications, and those who did not receive the warning barely changed them: in 2022, 1.7 percent of applicants below the threshold added a school, against 9.3 percent of warned applicants, and the discontinuity at the threshold points the same way as in Chile, although it is imprecise in narrow windows. In 2022 the jump at the threshold appears among families whose report card carried the warning and not among those warned by WhatsApp alone, although the difference between the arms is imprecise. In 2021, warned families were 9.5 percentage points less often unplaced in the first round than comparable families who were never contacted, with the largest reductions at the highest baseline risk, and simulating the assignment shows 110 more children placed overall, net of the placements other applicants lost. These comparisons are not randomized, and the 2021 comparison group could not revise. In Chile, follow-up work associates the short-run responses with gains in academic achievement up to five years after placement ([Arteaga et al., 2026](#)).

Third, recommendations change which schools families add. In the exploration set out in our pre-analysis plan, registered after the first Peruvian wave, the randomized suggestion list raised the share of Peruvian applicants adding a listed school by 6.3 percentage points among all assigned applicants in the 2021 cohort (7.4 with popularity and congestion information) and by 12.6 among families who opened the report card, with no significant change in how many schools they added. The three-school list did the same in 2022. Among openers, the ten-school list lowered simulated non-placement risk by 2.9 percentage points across the two cohorts, mainly in 2021, but neither pre-specified primary outcome shows an intention-to-treat effect of the list significant even at the 10 percent level in any wave, so we read this evidence as suggestive. The list appears to work mainly

by redirecting search that families have already begun.

The contrast between Peru and Ecuador shows that the same information, fielded by two ministries in the same year, can produce a clear response in one market and a weak one in another. In Ecuador the warning appeared only on the report card and reached fewer families, and warned families who opened it revised less often than in Peru. The list had no detectable effect, even among families who opened the report card, and the shortfall relative to Peru’s first cohort holds among openers whose card carried the warning, so reach is not the reason. Search was also narrower: few Ecuadorian families added schools, and those who did mostly chose schools the list would have named. Ecuadorian families were also choosing a school for the first time, on a platform open to revision throughout, without a separate warning message, and on phones the card may have placed the list behind an expand button. Ecuador’s market was thinner as well, although Peru’s second cohort, in the densest market, had a ten-school effect indistinguishable from Ecuador’s. Our design cannot say which of these differences mattered. Because outreach covered a large share of each market rather than a small treated subset, the setting is close to policy scale; the open equilibrium question is what stronger treatment intensity would do to congestion patterns.

The evidence has limits: the comparisons for the warning are not randomized, the 2022 discontinuity is imprecise, and the effects of the list come mainly from one cohort. The potential gains are large nonetheless. Families with the smallest consideration sets and the most optimistic beliefs are the ones a warning and a short list are built to reach, and where congested markets still have under-subscribed seats, prompting at-risk families to add schools fills seats that would otherwise stay empty while children go unplaced.

The interventions were embedded in government systems and run on live platforms. The comparison suggests a pre-rollout diagnostic: before projecting benefits, measure the frictions, how families already search, whether the ministry’s messages reach them, what alternatives the market offers, how much risk messaging officials will accept, and what delivery costs. All of them can be measured before a rollout.

Declarations

Funding

This research was funded by J-PAL Latin America and the Caribbean and by the Inter-American Development Bank. Staff of the Inter-American Development Bank contributed to the study design and to coordination with government partners, and two of them are authors of this article (see the CRediT statement below); beyond the authors' own contributions, the funders had no role in the analysis, the interpretation of results, or the decision to publish. No author receives compensation tied to the findings.

Declaration of Competing Interest

The interventions studied here were delivered on centralized school-choice platforms built and operated by ConsiliumBots, a non-profit technology organization that partners with Ministries of Education in Latin America. Christopher A. Neilson is the founder of ConsiliumBots; Thomas Krussig is affiliated with it; Felipe Arteaga was previously affiliated. As a non-profit it has no shareholders, and none of these relationships carries an equity or ownership interest. Gregory Elacqua and Carolina Méndez are staff of the Inter-American Development Bank, which funded implementation and survey costs through its Education Division and whose staff contributed to study design and to coordination with government partners. ConsiliumBots received no compensation beyond cost recovery for technical services provided to MINEDU and MINEDUC in connection with this study. No author has received honoraria, consulting fees or travel reimbursement from any party with a financial interest in the findings. The governments of Peru and Ecuador had no right to review or approve the manuscript before submission, and no party other than the authors had the right to delete, modify or veto any analysis or finding reported here.

Ethics Approval and Consent

The study was approved by Princeton University's Institutional Review Board (Office of Research Integrity and Assurance, protocols 13568 and 13618) and by

the Inter-American Development Bank Ethics Committee. Participants provided informed consent before taking part in the survey. Report-card extracts reproduced in the paper are illustrative: applicant and student names, distances from the home address and the exact home location have been redacted, and no personally identifying information is disclosed in this manuscript.

Trial Registration

The study is registered in the American Economic Association’s RCT Registry as AEARCTR-0007122 (<https://www.socialscienceregistry.org/trials/7122>, DOI <https://doi.org/10.1257/rct.7122-2.0>). The pre-analysis plan is dated February 2021 and the registry entry was filed on March 1, 2021. Only the Peru 2022 wave was fielded entirely after registration. The Peru 2021 wave was fielded in January 2021, and the Ecuador 2021 report cards were sent between February 25 and March 3, 2021, most of them on February 26, so neither 2021 wave was pre-registered; the plan is dated February 2021, the month in which most Ecuador report cards were sent. The Peru 2022 wave’s 30% risk threshold, three-school arm and restriction to two districts were operational decisions taken after registration. Analyses the plan did not specify are identified as exploratory in the text.

Data Availability

The analysis uses restricted microdata. The application, assignment and enrollment records for Peru were provided by the Ministry of Education of Peru (MINEDU) under a signed data-use commitment, and those for Ecuador were obtained through the Inter-American Development Bank’s technical cooperation with the Ministry of Education of Ecuador. The survey data were collected in partnership with both ministries under IRB-approved protocols whose consent terms restrict access to authorized personnel. These terms do not permit redistribution of the microdata, including de-identified extracts.

What will be deposited, in a public repository with a DOI and cited in this article: all code and documentation needed to reproduce every table and figure in the paper and appendix from the restricted data, and the pre-analysis plan. A

replication README will document the pipeline and software versions.

Researchers may request the microdata from MINEDU (<https://www.gob.pe/minedu>) and from the Ministry of Education of Ecuador (<https://educacion.gob.ec>) through the Inter-American Development Bank; the authors will supply documentation and code to support such requests. Estimated time from request to access is six to twelve months. The corresponding author will assist qualified researchers with the request process, and the authors will make the code and documentation available to the editors or referees on request during review. Appendix I documents the data sources.

CRediT Author Contributions

Felipe Arteaga: conceptualization, methodology, formal analysis, software, writing – original draft. **Gregory Elacqua:** conceptualization, investigation, project administration, funding acquisition, writing – review and editing. **Thomas Krusig:** software, data curation, investigation, writing – review and editing. **Carolina Méndez:** investigation, project administration, data curation, writing – review and editing. **Christopher A. Neilson:** conceptualization, methodology, formal analysis, supervision, funding acquisition, writing – original draft. All authors read and approved the submitted manuscript.

Declaration of Generative AI and AI-Assisted Technologies in the Manuscript Preparation Process

During the preparation of this work the authors used Anthropic’s Claude models, through the Claude Code command-line tool, to draft and revise prose, repair bibliographic entries, and write, check and debug analysis code, including the code that generates the tables and figures from the project’s data. The report-card screenshots shown in the paper were masked for privacy (names, distances and home locations) with the same assistance; no image content was created by a model. All empirical results derive from the authors’ own data and research design; no result, estimate, data point or image was generated by a model, and every analysis script produced with model assistance was reviewed and executed by the authors against the underlying data. After using these tools the authors

reviewed and edited the content as needed and take full responsibility for the content of the published article.

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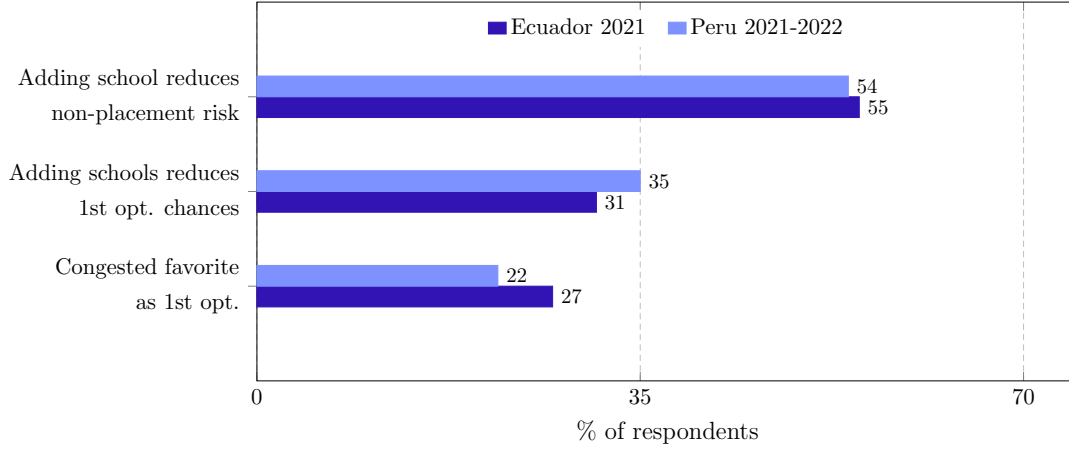
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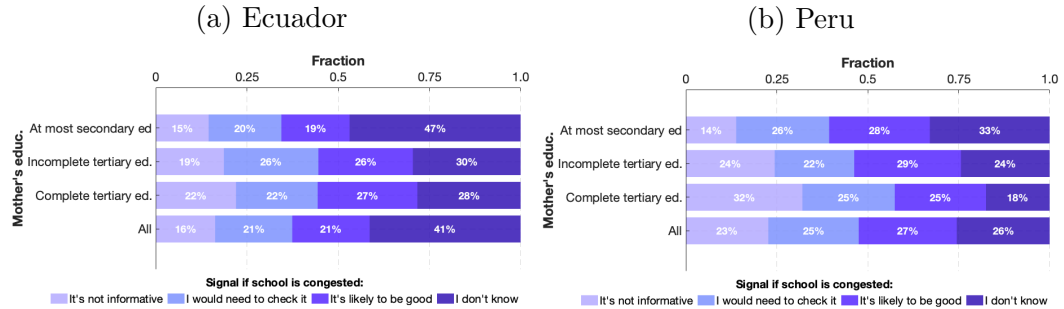
A Additional Figures

Figure A1: Understanding of the Mechanism



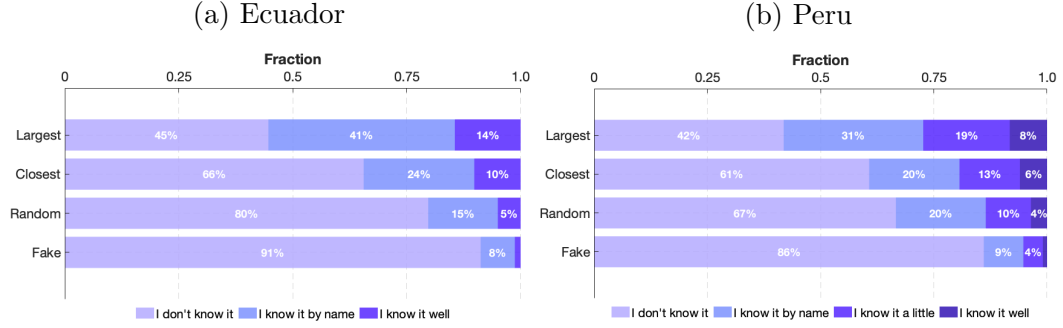
Notes: This figure shows the percentage of survey respondents who correctly answered questions about the deferred acceptance mechanism. Bars indicate the share answering correctly; the complement answered incorrectly. “Adding school reduces non-placement risk”: correct answer is yes (54–55% answered correctly). “Adding schools reduces 1st opt. chances”: correct answer is no—under deferred acceptance, adding schools cannot harm placement at higher-ranked options (only 31–35% answered correctly, implying 65–69% hold the incorrect belief). “Congested favorite as 1st opt.”: correct answer is yes (22–27% answered correctly). Data shown for Ecuador 2021 and Peru 2021–2022.

Figure A2: Inference From a Popular School



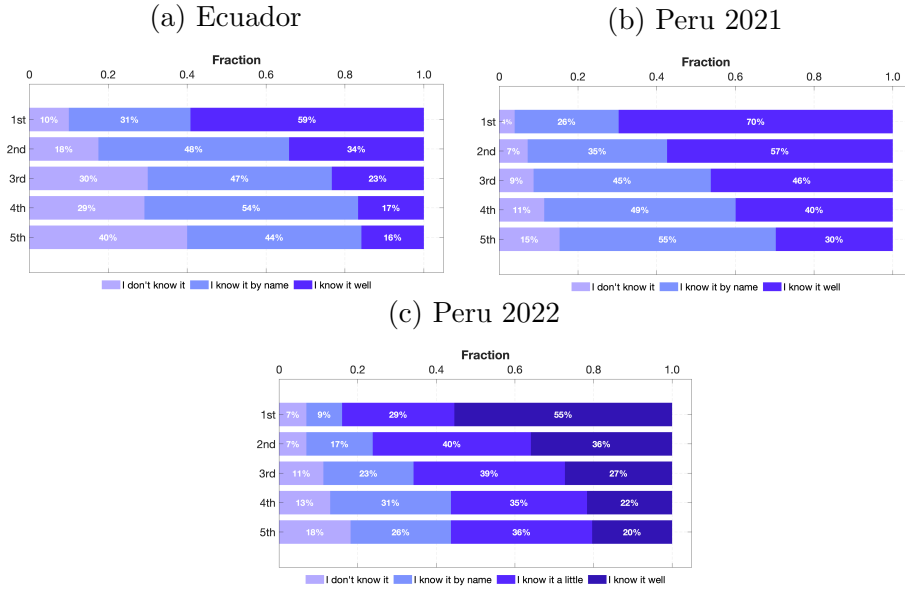
Notes: Panels (a) and (b) show responses to “If there’s a school that many families apply to, but you haven’t listed, what would you say about its quality that...” Section 5 describes the survey questions.

Figure A3: Knowledge About Private Options



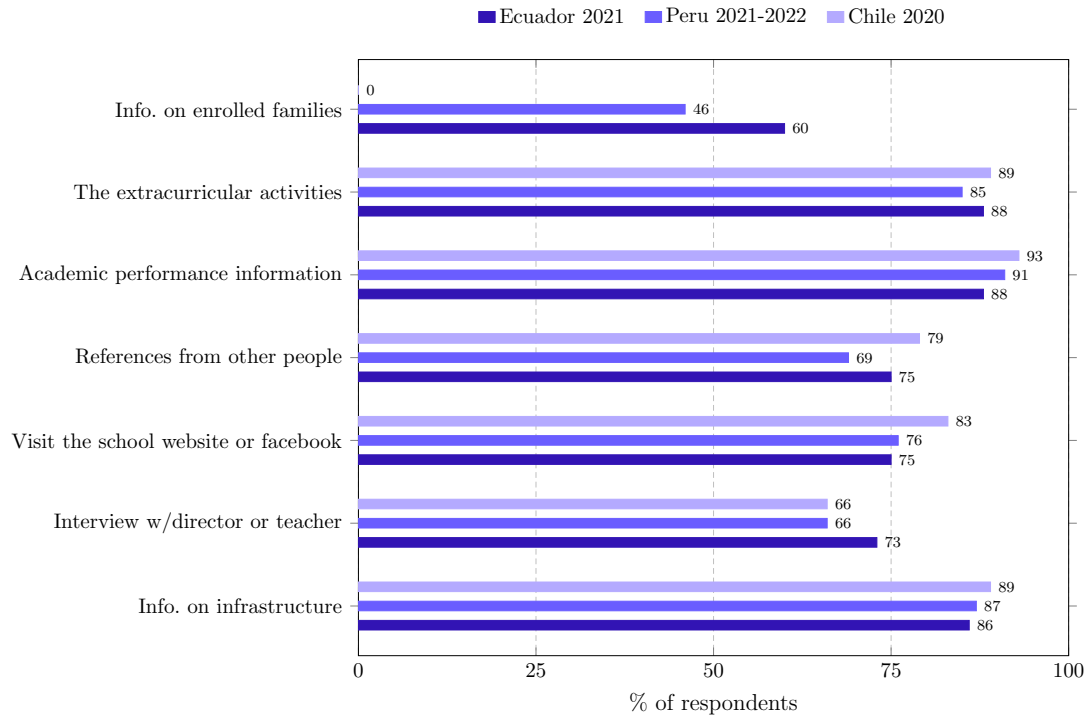
Notes: Panels (a) and (b) indicate familiarity with three private schools within 3 km of home (not on the platform), the largest, the closest and one drawn at random, and with a non-existent school name (“Fake”) as a check on over-reporting. Peru 2021 offered three answers and Peru 2022 four (adding “a little”); panel (b) splits the 2021 “by name” share equally between “by name” and “a little” and averages the two waves. Section 5 describes the survey questions.

Figure A4: Knowledge About Options in Application



Notes: Panels (a) to (c) show the level of knowledge of the schools in the application (question 6 of the survey), among respondents who answered it for all five ranked schools (120 in Ecuador, 333 in Peru 2021 and 231 in Peru 2022).

Figure A5: Necessary Steps for Learning about a School



Notes: The figure displays the answer to the question of the necessary steps for learning about a school (question 4 of the survey).

Figure A6: Report Card Example of T_2 for Peru 2022

¡Hola [Nombre]!

En esta cartilla informativa resumimos tus solicitudes en la actual ronda del proceso de Matriculación Digital.

↓

[NOMBRE DEL ESTUDIANTE]

Resumen de tu postulación

2 son los colegios que postulaste 1 colegio postulados están a menos de 2 km de tu casa 0 de los colegios postulados son integrados

Estos son los establecimientos a los que postulaste

NOMBRE DE ESCUELA	DISTANCIA	INTEGRADA	GÉNERO
1. I.E. 43001 Hermanos Barreto Calle Alto De Lima 1221 Tacna	[km]	☐	MIXTO
2. I.E. 42011 Republica Argentina Calle Chiclayo 310 Tacna	[km]	☐	MIXTO

INTEGRADA *
Una escuela se considera integrada si ofrece más de un nivel educativo, es decir una escuela que ofrece: educación inicial y primaria, educación primaria y secundaria o educación inicial, primaria y secundaria.

Has elegido colegios muy demandados

Hemos detectado que muchas familias han elegido los mismos colegios que tú.

Estos colegios no cuentan con vacantes suficientes para todos. Por lo tanto, existe la posibilidad de que no obtengas una vacante en ellos.

Para aumentar las posibilidades de obtener una vacante, agrega a tu solicitud a todos los colegios a los que estarías dispuesto a ir.

Hemos encontrado algunos colegios que podrían interesarte

En el siguiente mapa puedes ver la ubicación de los colegios en tu zona.

Este eres tu en el mapa

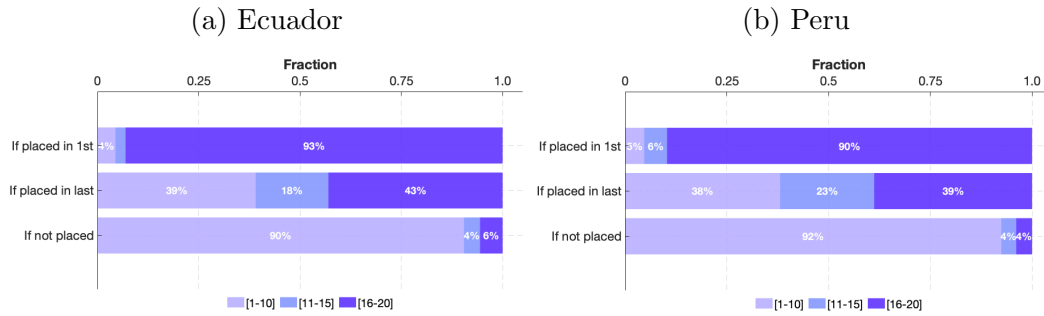
Muestra establecimientos que no incluye en tu postulación

Compara algunas alternativas de escuelas que podrías agregar a tu lista

NOMBRE DE ESCUELA	DISTANCIA	INTEGRADA	GÉNERO
1. I.E. 42198 Victor Raul Haya De La Torre Avenida Aviacion S/N Alto de La Alianza		☐	MIXTO
2. I.E. 42218 Mariscal Cáceres Avenida Internacional 1249 Ciudad Nueva		☒	MIXTO
3. I.E. 42005 Jose Rosa Ara Jose Rosa Ara 1840 Tacna		☐	MIXTO
4. I.E. 42012 Rebeca Martínez De Sánchez Avenida Celestino Vargas 2330 Tacna		☐	MIXTO
5. I.E. Guillermo Azañe Anco Avenida El Sol S/N Alto de La Alianza		☒	MIXTO
6. I.E. 42088 Don Jose De San Martín Avenida Juan Moore 312 Alto de La Alianza		☐	MIXTO
7. I.E. Federico Barreto Avenida Industrial 2710 Pocollay		☐	MIXTO
8. I.E. 42020 Alimentante Miguel Grau Calle Prolongacion Olga Grehmann S/N Tacna		☐	MIXTO
9. I.E. 43007 Luis Bencharo Rossi Avenida Mariscal Castilla 2015 Tacna		☐	MIXTO
10. I.E. 43004 Justo Arías Aránguez Calle Cahulide 400 Pocollay		☐	MIXTO

Notes: The panels illustrate a report card sent to an applicant in the 2022 Peru application process who was assigned to T_2 (warning + suggestion list) based on their positive placement risk. Applicant and student names, distances from the home address, and the exact home location have been redacted; the shaded disc in panel (b) covers an area of roughly 600 meters around the household, and suggested-school markers falling inside it are not shown. School names and addresses are those of public institutions.

Figure A7: Satisfaction with placement by rank



Notes: Panels (a) and (b) asked about the level of satisfaction for three scenarios: placed in first choice, last choice, and no placement (“If [applicant name] gets a spot in the following schools, from 1 to 20, how satisfied would you be?”). Responses are on a 1–20 scale, grouped as Low [1–10], Mid [11–15], and High [16–20]. Section 5 describes the survey questions.

B Additional Tables

Table A1: Summary of Information Interventions

	Ecuador 2021			Peru 2021			Peru 2022		
	T_1	T_2	T_{3-2021}	T_1	T_2	T_{3-2021}	T_1	T_2	T_{3-2022}
Target population	Applicants regardless of predicted placement risk			Applicants with predicted placement risk greater than 0%			Applicants with predicted placement risk greater than 30%		
<i>A. Sent by email</i>									
Link to report card	x	x	x	x	x	x			
<i>B. Sent by WhatsApp</i>									
Link to report card	x	x	x	x	x	x	x	x	x
Non-placement warning				x	x	x	x	x	x
<i>C. Included on the report card</i>									
Non-placement warning	x ^a	x ^a	x ^a	x	x	x		x	x
Suggestion list of up to 10 schools		x			x			x	
Suggestion list of up to 10 schools w/info on popularity and congestion			x			x			
Suggestion list of 3 schools									x

Notes. This table shows the target populations and different contents of our information intervention depending on the context. “Placement risk” is equivalent to non-placement probability. ^aIn Ecuador 2021 the report card carried the warning only for the 994 of 2,021 applicants (49%) whose simulated non-placement risk was at least 1%; the other 1,027 had simulated risk below 1% (932 of them exactly zero) and received the card without it.

Table A2: RCT Aggregate Results: Effect of Suggestions on Application Outcomes

Context	(1)	(2)	(3)	(4)
	T_1	All T_2	Peru 2021 T_1	and 2022 T_2
Intervention	Card	Card + list (10)	Warning	Warning + list (10)
	(base)	(diff.)	(base)	(diff.)
<i>A. Choice behavior</i>				
Add any school	0.207	0.028 (0.020)	0.271	0.037 (0.027)
Net schools added	0.540	0.124* (0.075)	0.711	0.123 (0.097)
Add popular	0.153	0.026 (0.018)	0.210	0.038 (0.025)
Add congested	0.192	0.017 (0.019)	0.253	0.025 (0.027)
<i>B. Add schools from list</i>				
Add from list (10)	0.117	0.069*** (0.017)	0.139	0.099*** (0.023)
Add outside list (10)	0.180	-0.028 (0.018)	0.245	-0.040 (0.025)
Add popular from list (10)	0.046	0.024** (0.012)	0.054	0.033** (0.016)
Add congested from list (10)	0.094	0.039** (0.016)	0.109	0.070*** (0.021)
<i>C. Assignment and Enrollment Outcomes</i>				
Placement risk	0.326	-0.012 (0.010)	0.376	-0.030** (0.013)
Placed in list (10)	0.035	0.015 (0.010)	0.040	0.026* (0.014)
Enrolled in list (10)	0.148	0.002 (0.019)	0.143	0.031 (0.027)
Observations	2,480	2,480	1,601	1,601

Notes. Columns 1 and 3 describe applicants assigned to T_1 (report card without a list): the before-and-after change in the portfolio in Panels A and B, and the level of each assignment and enrollment outcome in Panel C, a descriptive benchmark rather than a causal effect; columns 2 and 4 report the difference between T_2 (the report card with the ten-school list, “list (10)”) and T_1 . The regressions also include an indicator for assignment to a T_3 arm, which is not reported. Columns 1 and 2 include Ecuador 2021, where the report card carried the warning only for the 994 of the 2,021 assigned applicants whose simulated non-placement risk was at least 1%. The sample is applicants who opened the report card. The Observations row applies to Panels A and C. Panel B outcomes, defined only for arms shown the ten-school list, use 2,300 (columns 1–2) and 1,421 (columns 3–4) observations, excluding the 180 Peru 2022 applicants assigned the three-school list; “Enrolled in list (10)” uses 1,955 and 1,076, because enrollment is not observed for Peru 2022. In Ecuador, whose lists excluded ranked programs (school by shift) rather than schools, “Placed in list (10)” and “Enrolled in list (10)” count only listed schools not on the initial application (`codeAnalysis/ecuador_list_outcomes.py`). Linear regressions with controls for grade, district, placement risk and gender (Appendix E.1). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Baseline Balance by Treatment Arm: All Assigned Applicants

	Ecuador 2021			Peru 2021			Peru 2022		
	T_1 mean	$T_2 - T_1$	$T_3 - T_1$	T_1 mean	$T_2 - T_1$	$T_3 - T_1$	T_1 mean	$T_2 - T_1$	$T_3 - T_1$
Female	0.497	0.011 (0.027)	-0.000 (0.027)	0.458	0.021 (0.030)	0.032 (0.030)	0.541	-0.036 (0.036)	0.031 (0.036)
From largest district	0.707	0.006 (0.025)	0.004 (0.025)	0.579	-0.006 (0.029)	-0.002 (0.029)	0.650	0.000 (0.035)	-0.005 (0.035)
Pre-treatment portfolio length	1.809	-0.011 (0.073)	-0.038 (0.072)	2.956	0.027 (0.064)	0.026 (0.065)	2.862	-0.054 (0.066)	0.020 (0.066)
Placement risk at assignment				0.485	-0.003 (0.013)	-0.003 (0.013)	0.607	-0.003 (0.013)	-0.003 (0.013)
PPK (3 years old)	0.428	-0.001 (0.027)	-0.000 (0.027)	0.371	0.003 (0.029)	0.000 (0.029)	0.273	-0.005 (0.032)	0.011 (0.033)
PK (4 years old)	0.453	0.004 (0.027)	0.003 (0.027)				0.114	-0.001 (0.023)	-0.004 (0.023)
K (5 years old)	0.120	-0.002 (0.018)	-0.002 (0.018)				0.029	0.002 (0.012)	0.005 (0.013)
1st (6 years old)				0.629	-0.003 (0.029)	0.000 (0.029)	0.584	0.003 (0.036)	-0.012 (0.036)
Usable WhatsApp number	0.207	-0.002 (0.022)	0.041* (0.023)						
Opened the report card	0.395	0.029 (0.027)	0.092*** (0.027)	0.600	0.040 (0.029)	0.049* (0.029)	0.483	-0.054 (0.036)	-0.013 (0.036)
Joint test of all covariates, p -value		1.00	0.72		0.98	0.93		0.95	0.99
Observations	676	673	672	568	572	568	377	380	383

Notes: Each wave's columns report the T_1 mean and the raw $T_2 - T_1$ and $T_3 - T_1$ differences from a regression of the covariate on arm indicators, HC1 standard errors in parentheses, for every applicant assigned to an arm (Peru 2022: above the 0.30 threshold, as in every Peru 2022 column of the paper). Assignment was stratified on grade, district, risk bin and gender, so the raw differences test whether randomization produced comparable arms; within strata those four are balanced by construction. Placement risk at assignment is the deferred-acceptance risk recomputed on the initial list (Peru 2021) and the operational score that governed the report card (Peru 2022); Ecuador's assignment-time score is not in the analysis data, so that cell is blank; Section 7 quotes the risk printed on the Ecuadorian report cards. A usable WhatsApp number is shown only where it varies (every Peruvian applicant was sent a message); grade rows are blank where a wave had no applicants in that grade. "Joint test" is the p -value of a Wald test (HC1) that the covariates jointly do not predict assignment to the arm relative to T_1 , one grade category omitted and non-varying covariates dropped. T_3 is T_{3-2021} in Ecuador and Peru 2021 and T_{3-2022} in Peru 2022. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Baseline Balance by Treatment Arm: Report-Card Openers

	Ecuador 2021			Peru 2021			Peru 2022		
	T_1 mean	$T_2 - T_1$	$T_3 - T_1$	T_1 mean	$T_2 - T_1$	$T_3 - T_1$	T_1 mean	$T_2 - T_1$	$T_3 - T_1$
Female	0.494	0.032 (0.043)	0.013 (0.041)	0.496	-0.034 (0.038)	-0.024 (0.038)	0.582	-0.061 (0.054)	-0.005 (0.052)
From largest district	0.704	0.050 (0.038)	0.027 (0.037)	0.569	0.019 (0.037)	0.033 (0.037)	0.626	0.042 (0.052)	0.018 (0.051)
Pre-treatment portfolio length	1.914	-0.040 (0.121)	-0.051 (0.118)	2.994	0.000 (0.082)	0.022 (0.085)	2.912	-0.115 (0.098)	-0.112 (0.094)
Placement risk at assignment				0.501	-0.032* (0.017)	-0.011 (0.016)	0.613	-0.003 (0.019)	0.002 (0.018)
PPK (3 years old)	0.401	-0.001 (0.042)	0.012 (0.041)	0.370	0.008 (0.036)	0.004 (0.036)	0.286	-0.016 (0.048)	0.042 (0.049)
PK (4 years old)	0.464	-0.008 (0.043)	-0.006 (0.041)				0.088	0.029 (0.033)	0.023 (0.032)
K (5 years old)	0.135	0.009 (0.030)	-0.006 (0.028)				0.038	-0.014 (0.019)	0.006 (0.021)
1st (6 years old)				0.630	-0.008 (0.036)	-0.004 (0.036)	0.588	0.001 (0.053)	-0.071 (0.052)
Usable WhatsApp number	0.341	-0.025 (0.040)	0.014 (0.039)						
Joint test of all covariates, p -value		0.78	0.98		0.37	0.88		0.72	0.80
Observations	267	285	327	341	366	369	182	163	180

Notes: As in Table A3, for the applicants who opened the report card, the sample of Tables A2 and 8. Opening is post-randomization, so this panel tests whether selection into opening differed by arm on observables. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: RCT Results: Effect of Suggesting a School on the Probability of Adding it

Context	(1)	(2)	(3)	(4)
	All		Peru 2021 and 2022	
Intervention	T_1	T_2	T_1	T_2
	Warning	Warning + list (10)	Warning	Warning + list (10)
	(base)	(diff.)	(base)	(diff.)
Adding a school	0.019	0.016*** (0.004)	0.019	0.023*** (0.005)
Adding a risky school	0.029	0.023*** (0.006)	0.029	0.036*** (0.008)
Adding a high popularity school	0.044	0.034*** (0.010)	0.051	0.050*** (0.012)
Adding a low popularity school	0.005	0.004 (0.003)	0.002	0.005** (0.002)

Notes. This table shows predicted probability of adding a school from the personalized list for the three settings combined (columns 1 and 2) and only interventions in Peru (columns 3 and 4). Observations are school-level (applicant $\times$ school) with standard errors clustered on the applicant, and each row is estimated on a different subset of those pairs, so the sample size varies by row rather than by column: 23,072 and 14,655 for “Adding a school”, 9,026 and 5,868 for “Adding a risky school”, 4,344 and 2,957 for “Adding a high popularity school”, and 9,103 and 5,452 for “Adding a low popularity school” (columns 1–2 and columns 3–4 respectively). The sample is the applicants who opened the report card; in Peru 2022 it is restricted to those above the 0.30 threshold, the only ones shown a list (`codeAnalysis/school_level_effects.py`). Before the treatment, we selected a set of up to 10 schools to suggest to each applicant. We showed this list to a randomly selected sample. Columns (1) and (3) shows estimates of the probability of adding a school from the list conditional on not showing the list (T_1). Columns (2) and (4) show estimates of the causal effect of showing a school in the report card (T_2) on the probability of adding it. All the applicants in the sample also received a recommendation to add more schools, what we call the warning, except in Ecuador 2021 (pooled in columns 1 and 2), where the report card carried it only for the 994 of the 2,021 assigned applicants whose simulated non-placement risk was at least 1%. Estimates of the probabilities come from a linear probability model where the dependent variable is an indicator if the school was added to the portfolio, see Section E.2 in Appendix E for details on the model. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: RCT Results: Effect of Suggesting a School on the Probability of Adding it

Context	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ecuador 2021			Peru 2021			Peru 2022		
Intervention	T_1	T_2	T_{3-2021}	T_1	T_2	T_{3-2021}	T_1	T_2	T_{3-2022}
	Card	Card + list (10)	Card + list (10) + info pop/cong	Warning	Warning + list (10)	Warning + list (10) + info pop/cong	Warning	Warning + list (10)	Warning + list (3)
	(base)	(diff.)	(diff.)	(base)	(diff.)	(diff.)	(base)	(diff.)	(diff.)
Adding a school	0.018	0.003 (0.007)	-0.004 (0.006)	0.026	0.027*** (0.006)	0.020*** (0.006)	0.005	0.011** (0.005)	0.027** (0.011)
Adding a risky school	0.028	-0.003 (0.010)	-0.005 (0.009)	0.039	0.040*** (0.010)	0.032*** (0.009)	0.011	0.024** (0.012)	0.049** (0.020)
Adding a high popularity school	0.028	-0.001 (0.013)	-0.014 (0.011)	0.073	0.059*** (0.017)	0.028* (0.016)	0.009	0.021* (0.011)	0.044** (0.019)
Adding a low popularity school	0.012	0.001 (0.006)	-0.002 (0.005)	0.001	0.006** (0.002)	0.005** (0.003)	0.004	0.003 (0.005)	0.013 (0.010)

Notes. This table shows predicted probability of adding a school from the personalized list for Ecuador 2021 (columns 1 to 3), Peru 2021 (columns 4 to 6), and Peru 2022 (columns 7 to 9). The sample is the applicants who opened the report card; in Peru 2022 it is restricted to those above the 0.30 threshold, the only ones shown a list (`codeAnalysis/school_level_effects.py`). Before the treatment, we selected a set of up to 10 schools to suggest to each applicant. We showed this list to a randomly selected sample. Columns (1), (4), and (7) show estimates of the probability of adding a school from the list conditional on not showing the list (T_1). Columns (2), (5), and (8) show estimates of the causal effect of showing a school in the report card (T_2) on the probability of adding it. Columns (3), (6), and (9) show estimates of the causal effect of T_3 on the probability of adding it. All the applicants in the sample also received a recommendation to add more schools, what we call the warning, except in Ecuador 2021 (“Card”), where the report card carried it only for the 994 of the 2,021 assigned applicants whose simulated non-placement risk was at least 1%. Estimates of the probabilities come from a linear probability model where the dependent variable is an indicator if the school was added to the portfolio, see Section E.2 in Appendix E for details on the model. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: RCT Results: Differences Between Treatments 2 and 3

Context	(1) Ecuador 2021	(2) Peru 2021	(3) Peru 2022
Intervention difference	$T_{3-2021} - T_2$ info pop/cong (diff.)	$T_{3-2021} - T_2$ info pop/cong (diff.)	$T_{3-2022} - T_2$ fewer schools (diff.)
<i>A. Choice behavior</i>			
Add any school	-0.018 (0.023)	-0.012 (0.035)	0.045 (0.040)
Net schools added	-0.225** (0.112)	-0.116 (0.121)	0.023 (0.184)
Add popular	-0.023 (0.016)	-0.076** (0.033)	0.037 (0.037)
Add congested	-0.017 (0.021)	-0.018 (0.035)	0.037 (0.039)
<i>B. Add schools from list</i>			
Add from list (10)	-0.016 (0.022)	-0.028 (0.033)	-0.027 (0.028)
Add outside list (10)	-0.025 (0.016)	-0.019 (0.032)	
Add popular from list (10)	-0.017 (0.014)	-0.026 (0.021)	
Add congested from list (10)	-0.001 (0.017)	-0.014 (0.031)	
Placement risk	0.002 (0.012)	0.027 (0.017)	-0.015 (0.019)
<i>C. Assignment and Enrollment Outcomes</i>			
Placed in list (10)	0.000 (0.013)	-0.020 (0.019)	-0.003 (0.016)
Enrolled in list (10)	-0.003 (0.022)	-0.017 (0.026)	
Observations	879	1,076	525

Notes. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The reported sample size applies to every row. The three list-composition outcomes (“Add outside list (10)”, “Add popular from list (10)” and “Add congested from list (10)”) and “Enrolled in list (10)” are not reported for Peru 2022, where the two arms saw lists of different length and enrollment is not observed. This table shows the differential effect of treatments T_3 versus T_2 of Ecuador 2021 (column 1) and Peru (columns 2 and 3). For Ecuador 2021 and Peru 2021 (columns 1 and 2) the difference between T_{3-2021} and T_2 is that the former included information in popularity and congestion for each school in the list. (“info pop/cong”). For Peru 2022 the difference between T_{3-2022} and T_2 is that the former included 3 schools and the latter 10 schools; in column 3, “Add from list (10)” compares adding a school from each arm’s own list (three schools in T_{3-2022} , ten in T_2). The sample considers only applicants that opened the link to the report card. As in Table 7, the Ecuadorian “Placed in list (10)” and “Enrolled in list (10)” count only listed schools not on the initial application.

Table A8: RCT Results: Effect of Suggestions on Subjective Outcomes

Country	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	T_1	T_2	T_3	T_1	T_2	T_3	T_1	T_2	T_3
Intervention	Card	+ list (10)	+ list (10) + info pop/cong	Warning	+ list (10)	+ list (10) + info pop/cong	Warning	+ list (10)	+ list (3)
	(base)	(diff.)	(diff.)	(base)	(diff.)	(diff.)	(base)	(diff.)	(diff.)
<i>A. Application process</i>									
Was it difficult to search for schools?	0.279 (0.041)	-0.056 (0.056)	-0.003 (0.054)	0.408 (0.057)	-0.155** (0.076)	-0.182** (0.073)	0.353 (0.067)	-0.031 (0.091)	0.001 (0.090)
Evaluation of school info on platform [1 to 20]	17.913 (0.277)	-1.292*** (0.496)	-0.059 (0.381)	13.277 (0.560)	0.944 (0.726)	0.930 (0.801)	14.556 (0.631)	0.401 (0.827)	0.376 (0.862)
General evaluation of process [1 to 20]	18.396 (0.239)	-0.534 (0.417)	-0.217 (0.351)	13.584 (0.542)	-0.336 (0.756)	0.199 (0.774)	14.143 (0.658)	0.973 (0.837)	-0.198 (0.905)
Received warning	0.567 (0.044)	0.100 (0.061)	0.057 (0.059)	0.808 (0.045)	0.053 (0.060)	-0.008 (0.063)	0.451 (0.070)	0.216** (0.093)	0.231** (0.091)
Do not plan to apply to private	0.548 (0.043)	0.040 (0.061)	0.045 (0.057)	0.772 (0.047)	0.009 (0.065)	0.001 (0.065)	0.745 (0.062)	-0.078 (0.087)	0.103 (0.076)
<i>B. Satisfaction</i>									
Satisfaction if placed in 1st [1 to 20]	19.524 (0.194)	-0.260 (0.315)	-0.287 (0.297)	19.224 (0.187)	0.013 (0.340)	-0.306 (0.379)	19.137 (0.320)	-0.239 (0.445)	-0.106 (0.462)
Satisfaction if placed in last [1 to 20]	13.729 (0.706)	-1.244 (1.086)	0.298 (0.952)	12.882 (0.713)	-0.763 (0.980)	0.142 (0.981)	14.000 (0.784)	-0.610 (1.041)	-1.431 (1.050)
Satisfaction if no placement [1 to 20]	4.008 (0.449)	-0.438 (0.579)	0.479 (0.604)	2.053 (0.276)	0.816 (0.559)	0.289 (0.469)	3.137 (0.602)	-0.578 (0.754)	-0.953 (0.695)
<i>C. Knowledge and beliefs</i>									
Proportion of schools from list (10) that know	0.355 (0.024)	-0.074** (0.032)	-0.044 (0.031)	0.361 (0.041)	0.143** (0.056)	0.075 (0.057)	0.208 (0.035)	0.022 (0.048)	
Proportion of schools from list (3) card that know							0.270 (0.051)		0.079 (0.074)
Private schools that don't know (out of 5)	2.762 (0.145)	0.152 (0.207)	0.222 (0.193)	2.082 (0.187)	0.062 (0.260)	0.137 (0.263)	2.406 (0.237)	0.234 (0.329)	0.421 (0.311)
Subjective risk	0.081 (0.018)	-0.010 (0.023)	0.010 (0.023)	0.120 (0.016)	-0.010 (0.022)	0.007 (0.025)	0.107 (0.020)	0.006 (0.028)	0.001 (0.029)

Notes. Columns 1, 4 and 7 report the mean response among applicants assigned to T_1 (report card without a list); the other columns report differences from that mean. T_2 adds the ten-school list, “list (10)” (columns 2, 5 and 8); T_{3-2021} adds popularity and congestion information for each listed school, “info pop/cong” (columns 3 and 6); T_{3-2022} replaces the ten-school list with a three-school list, “list (3)” (column 9). The sample is applicants who opened the report card. In Ecuador 2021 (“Card”) the report card carried the warning only for the 994 of 2,021 assigned applicants (49%) whose simulated non-placement risk was at least 1%; the other 1,027 had simulated risk below 1% (932 of them exactly zero) and received the card without it. Sample sizes vary by question because of item non-response: 209 to 505 in Ecuador 2021, 235 to 347 in Peru 2021 and 111 to 225 in Peru 2022; the two list-knowledge rows are further restricted to the arm shown that list (111 and 112 observations in Peru 2022). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: ITT and TOT Decomposition of the Suggestion-List Contrast ($T_2 - T_1$)

Outcome	Ecuador 2021		Peru 2021		Peru 2022	
	ITT	TOT	ITT	TOT	ITT	TOT
Add any school	0.012 (0.012)	0.028 (0.029)	0.009 (0.028)	0.014 (0.043)	-0.001 (0.021)	-0.002 (0.048)
Net schools added	0.068 (0.052)	0.161 (0.123)	0.003 (0.090)	0.004 (0.141)	0.028 (0.069)	0.066 (0.161)
Add from list (10)	0.012 (0.011)	0.028 (0.026)	0.063*** (0.024)	0.098*** (0.037)	0.016 (0.013)	0.036 (0.030)
Report-card open rate, T_2 arm (first stage)	0.423		0.640		0.429	

Notes: ITT columns report the coefficient on the $T_2 - T_1$ differential from Table 7, for all assigned applicants. Standard errors in parentheses. TOT columns divide the ITT point estimate and its standard error by the share of T_2 applicants who opened the report card (Table 2, Panel E; 285 of 673 in Ecuador 2021, 366 of 572 in Peru 2021 and 163 of 380 in Peru 2022). Because no T_1 card carries a list, the T_2 opening rate is the first stage, and the ratio is the Wald estimate with assignment to T_2 as the instrument for opening a card that carries the list. It equals the list's effect on the families who open the card when assigned to T_2 only if assignment does not affect families who do not open it. For adding a listed school in Ecuador, applicants recorded as not opening the card show a $T_2 - T_1$ difference of 0.014 (s.e. 0.009), so the Ecuadorian TOT exceeds the direct estimate among openers (0.005, s.e. 0.024), and we treat the rescaling as descriptive. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, based on the underlying ITT test.

Table A10: Optimism Bias Across the Surveys: Means and Tests

Sample	N	Mean optimism	s.d.	s.e.
Ecuador 2021	195	0.374	0.404	0.029
Peru 2021	733	0.294	0.311	0.011
Peru 2022	560	0.197	0.295	0.012
Peru (pooled)	1293	0.252	0.308	0.009
Comparison	Difference	(s.e.)	Welch p	KS p
Peru (pooled) vs Ecuador 2021	-0.122	(0.030)	< 0.001	< 0.001
Peru 2021 vs Ecuador 2021	-0.080	(0.031)	0.011	< 0.001
Peru 2022 vs Ecuador 2021	-0.176	(0.031)	< 0.001	< 0.001
Peru 2021 vs Peru 2022	+0.096	(0.017)	< 0.001	< 0.001

Notes: Optimism is the true non-placement risk minus the subjective non-placement risk, for survey respondents with true risk above 0.01, the sample of Figure 4. The upper panel reports means with standard deviations and standard errors; the lower panel reports differences in means with their standard errors, the p -value of a Welch two-sample t -test and the p -value of a two-sample Kolmogorov-Smirnov test of equal distributions. Chile is not tested because its microdata are not part of this study and its survey instrument, timing and sampling frame differ. Source: `codeAnalysis/optimism.test.py`.

Table A11: The Pilot Regions and Chile: Applicants, Options and Placement Risk

<i>A. Markets and applications</i>				
	Ecuador 2021 (Manta)	Peru 2021 (Tacna)	Peru 2022 (Tacna)	Chile 2020 (national)
Applicants on the platform	3,984	6,876	4,856	454,226
Share applying in the largest district	0.66	0.43	0.40	–
On-platform options within 2 km	11.32	16.33	16.66	–
Off-platform options within 2 km	12.11	19.28	20.30	–
Schools listed on the initial application	1.83	3.14	3.16	–
Share ranking exactly one school	61%	0.1%	4.6%	–
<i>B. Simulated non-placement risk, all applicants</i>				
	Zero	(0, 0.3]	(0.3, 1]	Mean (P75)
Ecuador 2021 (Manta), $N = 3,984$	54.7%	15.1%	30.2%	0.24 (0.40)
Peru 2021 (Tacna), $N = 6,876$	43.3%	24.1%	32.6%	0.22 (0.46)
Peru 2022 (Tacna), $N = 4,856$	50.0%	27.7%	22.3%	0.15 (0.24)
Chile 2020 (national), $N = 454,226$	59.6%	13.7%	26.7%	0.22 (0.37)
Coquimbo - La Serena, $N = 13,994$	41.7%	14.5%	43.8%	0.36 (0.82)
Valparaíso - Viña, $N = 26,215$	60.3%	14.0%	25.7%	0.21 (0.33)
Rancagua, $N = 11,971$	58.2%	14.9%	26.9%	0.22 (0.38)
Concepción - Talcahuano, $N = 24,548$	53.2%	12.3%	34.5%	0.28 (0.59)
Puerto Montt - Puerto Varas, $N = 8,864$	51.4%	16.9%	31.8%	0.26 (0.54)
Punta Arenas, $N = 3,356$	52.6%	9.5%	37.9%	0.32 (0.77)
Santiago, $N = 158,057$	56.7%	15.2%	28.1%	0.22 (0.41)

Notes: Panel A: all applicants on each platform (Table 1, columns “All”); options are schools within 2 km of the home address; the one-school shares are from Section 3; the Chilean count is the 2020 platform’s applicants (Arteaga et al., 2022). Panel B: distribution of simulated non-placement risk among all applicants: for Ecuador and Peru the risk simulated for each applicant’s final application (`codeAnalysis/export_optimism.m`); for Chile the 2020 platform’s risk measure, nationally and for its seven largest urban areas (`codeAnalysis/createTable_risk_descriptive_stats.m`). Source: `codeAnalysis/region_summary_table.py`.

C Outreach and Treatment Details

The main channel of communication with families was through the messaging app WhatsApp. We used the cellphone numbers reported by the applicants in the registration process.

The messages we sent through the WhatsApp messaging app differed between the two contexts. In Peru, we sent a warning message about the possibility of not being assigned (WhatsApp warning), while in Ecuador we did not; there, the warning appeared only on the report card, and only for applicants whose simulated non-placement risk was at least 1%. In both settings, we sent reminders to check the link with the report card. An example of the messages we sent is displayed in Table [A12](#).

Earlier report-card batches in Ecuador. Two earlier batches of Ecuadorian report cards, generated on February 24 and 25, 2021, carried arm assignments unrelated to the one we analyze. In each of those batches every analyzed arm was split roughly equally across the three card contents, whereas every batch from February 26 onward showed each applicant it included the content of their analyzed arm. The earlier assignments were then discarded. According to Google Analytics, 60 of the 879 families who opened a card (6.8%) first visited the card site before February 26. If the analyzed cards were the ones online from the February 26 launch, these are the only families who could have seen a discarded assignment, and even if all of them had responded only to it, a Peru-sized list effect of 0.126 would be attenuated only to about 0.117. The bound rests on that assumption, which the card team’s file history supports (next paragraph): if the earlier cards had stayed online during the first hours of February 26, families who first visited that day (57% of openers) could also have seen them. Outcomes do not respond to the assignment in the February 25 batch, whether among the assigned applicants in that batch, among openers, or among openers split by first visit (coefficients between -0.026 and 0.025 , none significant), although these tests have limited power.

Display errors on the Ecuadorian launch day. The card team’s records document two display errors in the first hours after the February 26 launch. Until

3:10 pm (EST), the suggestions were listed in the order 1, 10, 2, 3, and so on, so the tenth suggestion appeared second; until 4:16 pm, the T_{3-2021} demand indicator showed the same value (3) for every school. A file update at 2:47 pm changed only the school-name columns, which supports the assumption behind the bound above that the analyzed assignment was served from launch; school names and shifts were updated on the evening of February 27. Google Analytics records first visits by day rather than by hour, so we cannot isolate the families who saw the cards during those hours. The list’s effect on adding a listed school is small both for openers who first visited by February 26 (0.010, s.e. 0.029) and for those who first visited later (-0.004 , s.e. 0.040). The negative T_{3-2021} estimate on schools added is, if anything, larger among openers who first visited after February 26 (-0.190 , s.e. 0.094, against -0.069 , s.e. 0.084).

Table A12: Example of WhatsApp Conversation from Peru 2022 - Translation to English

#	Name	Content
1	WhatsApp introduction	Hello [guardian name], we are writing to you since you are registered in the 2022 Digital School Enrollment System. We would like to share information with you regarding your application. Answer "Yes" to review it.
2	WhatsApp disclaimer	The answers you give us in this conversation are confidential and will not affect your application. Our aim is to help you to have more information so that you can submit a good application
3	WhatsApp warning	We have detected that many families have chosen the same schools as you! Many families are applying to the same schools as you, so there is a chance that you may not be placed at those schools. To increase your chances of being placed, add all the schools you would be willing to attend to your application.
4	WhatsApp with link to report card	In the following link, you will find important information regarding your application. [link to report card]
5	WhatsApp reminder	Remember that you can make changes to your application until December 26 at [link to application platform] The last application you send will be the valid application. If you change your mind, feel free to reflect this in your application.
6	WhatsApp closing	See you soon [guardian name], have a nice day

Notes. This table shows an example of the messages sent to applicants with an elevated placement risk. Messages 2 to 6 were sent only if the guardian answered positively to the first message (WhatsApp introduction). Applicants without an elevated placement risk received the same messages but no WhatsApp warning (message 3).

Table A13: Example of WhatsApp Conversation from Peru 2022 - Original Spanish

#	Name	Content
1	WhatsApp introduction	Hola [nombre apoderado], te escribimos dado que estás registrado en el Sistema de Matrícula Escolar Digital 2022. Quisiéramos compartir contigo información respecto a tu postulación. Contesta "Sí" para revisarla.
2	WhatsApp disclaimer	Las respuestas que nos entregues en esta conversación son confidenciales y no afectarán tu postulación. Buscamos ayudarte a que tengas más información para que realices una buena postulación
3	WhatsApp warning	Hemos detectado que muchas familias han elegido los mismos colegios que tú! Muchas familias están postulando a los mismos colegios que tu, por lo que existe la posibilidad de que no obtengas una vacante en ellos. Para aumentar las posibilidades de obtener una vacante, agrega a tu postulación todos los colegios a los que estarías dispuesto a ir.
4	WhatsApp with link to report card	En el siguiente enlace encontrarás información importante respecto a tu postulación. [link a cartilla]
5	WhatsApp reminder	Recuerda que puedes hacer cambios a tu postulación hasta el 26 de diciembre en [link plataforma de postulación] La última postulación que envíes será la postulación válida. Si cambias de opinión, no dudes en reflejarlo en tu postulación.
6	WhatsApp closing	Hasta pronto[nombre apoderado], que tengas un buen día

Notes. This table shows an example of the messages sent to applicants with an elevated placement risk. Messages 2 to 6 were sent only if the guardian answered positively to the first message (WhatsApp introduction). Applicants without an elevated placement risk received the same messages but no WhatsApp warning (message 3).

D Intervention Construction Details

For the warning, we used the same message as [Arteaga et al. \(2022\)](#), adding a “fire rating” symbol to show the level of risk. Figure 3a shows the warning as displayed in the report card when included in the assigned treatment. It contained the following message:

We have detected that many families are applying to the same schools as you, so there is the possibility that you will not be granted a spot in any of them.

Remember that to increase the chances of obtaining a spot, we recommend adding all the schools that you would be willing to attend to your application.

The school suggestions for T_2 and T_{3-2021} consisted of a list of up to 10 schools that the student did not include in their initial ROL, while T_{3-2022} included only three schools. We built each personalized list from nearby alternatives: 77% of Ecuadorian and 91% of Peru 2021 entries lie within 3 km of the declared home address, and no Ecuadorian entry lies beyond 5 km. In Ecuador, 23% of lists had fewer than ten entries, and a school offering two shifts could appear twice, so the lists named 8.3 distinct schools on average. The lists sent in T_{3-2021} included at least one popular *undersubscribed* school, one popular *oversubscribed* school, two non-popular *undersubscribed* schools with at least 5 applicants, and two non-popular *oversubscribed* schools with at least 5 applicants. To fill the remaining places, we added random schools from the student’s neighborhood.

To create proxies of popularity, we used the applications collected at the time of the intervention. We classified schools according to the number of applications. The minimum number to be considered “popular” was the number of applications received by the most demanded school with some available seats. This definition allowed us to classify at least one school as *undersubscribed* within the set of popular schools and, potentially, many *oversubscribed* schools.¹ The process was

¹We define a school as “*oversubscribed*” if the probability of a regular applicant being placed there is less than 100%, which is equivalent to having more demand than seats. A school is “*undersubscribed*” if every potential applicant to the school can be placed there. We follow the same procedure as in [Arteaga et al. \(2022\)](#) to obtain the placement probabilities for each school.

conducted at a district level, meaning that only applicants from the district were considered for the definition of popularity within each specific geographic zone.

The information provided to families who received lists of suggested schools included the school’s name, the distance from the applicant’s address on the application form, and the levels of education offered.² The Peruvian report card also included whether the school was single-sex or co-ed.

The information on popularity and congestion provided in T_{3-2021} incorporates two additional pieces of information for each of the 10 schools on the personalized suggestion list. The first was a discrete category called “popular”, which was based on the number of applications from the same district, as explained above. We displayed this on the report card as “High” or “Low” demand. The second additional component was the number of applicants and open seats available.³ Extracts of the report cards are shown in Figures 3a and 3b. The full report card is presented in Figure A6 in Appendix A.

²For Ecuador, the report card explicitly showed which educational levels were offered at the school (Inicial, EGB, and Bachillerato, which correspond to preschool, elementary, and high school, respectively). For Peru, this information was limited to whether or not the school was classified as integrated (integrado), meaning that it offered both preschool and some higher levels of education (e.g., preschool + elementary or preschool + elementary + high school).

³The number of applicants corresponds to the mean of the number of admitted plus waitlisted students from 500 simulations of the assignment based on the current demand. In this case, we did not differentiate from the applicant’s geographic origin, we included all applicants.

E Regression Model Details

E.1 Main Linear Regression Model

The estimates presented in Tables 8 and A2 are obtained from the following regression model, estimated on all three arms:

$$Y_i = \alpha + \beta T2_i + \theta T3_i + \sum_g \gamma_g \mathbf{1}[\text{Grade}_i = g] + \sum_d \delta_d \mathbf{1}[\text{District}_i = d] \\ + \sum_r \lambda_r \mathbf{1}[\text{RiskBin}_i = r] + \phi \mathbf{1}[\text{Gender}_i] + \varepsilon_i,$$

where Y_i is the outcome variable for individual i , $T2_i$ and $T3_i$ are indicators for assignment to T_2 and to a T_3 arm, and the model includes fixed effects for grade (γ_g), district (δ_d), and placement risk bin (λ_r), as well as a control for gender (ϕ).

The regression is estimated using heteroskedasticity-robust standard errors.

All controls are de-meant, so α represents the mean of the outcome variable for the omitted group (applicants assigned to T_1), which is reported in columns (1), (4) and (7) of Table 8 and columns (1) and (3) of Table A2. The coefficient β captures the differential effect of T_2 relative to T_1 , as shown in columns (2), (5) and (8) of Table 8 and columns (2) and (4) of Table A2.

Table 8 estimates the model separately for each wave, and θ , the differential effect of T_3 relative to T_1 , is reported in its columns (3), (6) and (9). Table A2 pools waves; there $T3_i$ marks assignment to either wave's T_3 arm, and its coefficient is not reported.

E.2 Linear Probability Model

The probability estimates in Table A5 are obtained from the following linear probability model:

$$Y_{ij} = \beta_0 + \beta_1 T2_i + \beta_2 \text{Distance}_{ij} + \beta_3 (\text{Distance}_{ij} \times T2_i) \\ + \beta_4 \text{Risky}_i + \beta_5 (\text{Risky}_i \times T2_i) + \beta_6 \text{CatPop}_j + \beta_7 (\text{CatPop}_j \times T2_i) + \varepsilon_i.$$

where:

- Y_{ij} equals 1 if applicant i added school j to their portfolio. School j is part of the personalized suggestions generated for applicant i , which were revealed only if the applicant was assigned to T_2 or T_3 .
- Distance_{ij} is a continuous variable measuring the distance from applicant i 's home to school j .
- Risky_j is a binary variable indicating whether the school has a non-placement risk greater than 0.
- CatPop_j represents a set of dummy variables for school popularity categories.
- Interaction terms allow for heterogeneous treatment effects based on distance, riskiness, and popularity category.
- Standard errors are clustered at the individual level.

After estimating this regression, we compute the average marginal effect of treatment T_2 on the probability of adding a school. This effect, representing the differential impact of T_2 relative to T_1 , is reported in columns (2) and (4) of Table A5.

All controls are de-meanned, so β_0 represents the mean outcome for the omitted group (applicants assigned to T_1), reported in columns (1) and (3) of Table A5.

The estimates in Table A6 (Appendix B) are based on a similar specification but include additional interaction terms for $T3_i$, where $T3_i$ is an indicator for assignment to T_3 . The average marginal effect of T_3 relative to T_1 is reported in columns (3), (6), and (9) of Table A6.

F Minimum Detectable Effects Conditional on Realized Reach

This appendix reports minimum detectable effect (MDE) calculations for the suggestion-list contrast ($T_2 - T_1$) discussed in Section 7. The purpose is to assess whether Ecuador’s attenuated list effects could be explained by low statistical power once the realized reach of the intervention is taken as given. These calculations were not part of the pre-analysis plan and should be read as an exploratory robustness exercise prompted by the gap between Peru and Ecuador in observed treatment effects.

For each wave $w \in \{\text{Ecuador 2021, Peru 2021, Peru 2022}\}$ and each outcome y , we compute the MDE at significance level $\alpha = 0.05$ (two-sided) and power $1 - \beta = 0.80$ using the published standard error of the $T_2 - T_1$ coefficient from our main ITT specification (Table 7):

$$\text{MDE}_{w,y} = (z_{1-\alpha/2} + z_{1-\beta}) \cdot \widehat{\text{SE}}_{w,y} \approx 2.80 \cdot \widehat{\text{SE}}_{w,y}.$$

The standard error is computed on the same data used for the ITT estimate, so it already incorporates the realized sample size and the realized report-card open rate in each wave. No additional adjustment is therefore required to obtain the MDE conditional on the reach actually achieved. We then compare each MDE to the Peru 2021 point estimate for the same outcome, which provides a simple benchmark for asking whether a Peru-sized effect would have been detectable.

Table A14 reports these MDEs for the three outcomes most directly affected by the list. The comparison is informative for “add from list (10),” the only outcome on which Peru 2021 registers a statistically significant list effect: Ecuador’s MDE is 0.031, about half of Peru’s point estimate of 0.063, so a Peru-sized effect in Ecuador would have been detectable at conventional significance levels. It is not informative for the other two outcomes, where the Peru 2021 estimates are themselves indistinguishable from zero (0.009 and 0.003) and every wave’s MDE exceeds them by a wide margin; nothing about power can be concluded from a benchmark that is itself a null. The same caveat applies to Peru 2022.

Ecuador’s near-null list effect is therefore unlikely to be a sample-size artifact.

Table A14: Minimum Detectable $T_2 - T_1$ Effects by Wave

Outcome	Ecuador 2021 MDE	Peru 2021 MDE	Peru 2022 MDE	Peru 2021 ITT estimate
Add any school	0.034	0.077	0.058	0.009
Net schools added	0.146	0.252	0.193	0.003
Add from list (10)	0.031	0.067	0.035	0.063***

Notes: MDE computed as $2.80 \cdot \text{SE}$ using the $T_2 - T_1$ coefficient standard error from Table 7. This corresponds to a two-sided test at $\alpha = 0.05$ with 80% power. Standard errors already reflect the realized sample size and the realized report-card open rate in each wave, so the MDE is conditional on the delivery reach actually achieved. The Peru 2021 ITT estimate in the final column is reported from the same table and is provided as a benchmark.

The 0.012 point estimate observed in Ecuador on “add from list (10)” is small relative to an MDE that would have allowed us to detect a Peru-sized effect. Among openers the estimate is 0.005 (s.e. 0.024), and it is unchanged without controls or in a logit specification (0.004, s.e. 0.023), with a randomization-inference p -value of 0.87. This supports the interpretation in Section 7: neither low delivery reach nor insufficient statistical power explains the Ecuadorian null, which arose in a wave where families searched little; other differences between the settings, including market structure, cannot be separated by the design. A display explanation, in which phones kept the list collapsed, cannot be ruled out with the data we have (Section 7).

G Robustness of the Peru 2022 Discontinuity

The discontinuity reported in Figure 7 is estimated at the bandwidth used for every discontinuity estimate in this paper, $h = (0.3, 0.7)$ around the 0.30 cutoff, on applicants in the list arms (T_2 and T_{3-2022}) whose report card was delivered, the same 939 applicants as the figure. At that bandwidth several outcomes are significant; at the MSE-optimal bandwidth of roughly 0.14 they are not. Because the choice is consequential we report the entire bandwidth path rather than defend one window, and add a placebo-cutoff test. Together they support the reading in Section 6: the discontinuity in schools added is positive but too imprecise to confirm, and the discontinuity in placement risk is not identified in this wave.

Table A15 traces each outcome across bandwidths. The point estimate for schools added is largest in the narrowest windows and settles near 0.4 as the window widens, but it is not significant at any fixed bandwidth between 0.05 and 0.5 or at the MSE-optimal bandwidth; it reaches significance only under the asymmetric window $h = (0.3, 0.7)$, which covers the whole risk support. The share adding any school is significant from a bandwidth of 0.3 upward (0.078, s.e. 0.035, at $h = 0.3$). The effects on simulated risk and on realized non-placement sit at zero near the cutoff and reach significance only once the bandwidth covers most of the support, which is what fitting a slope through a steeply sloped relationship produces.

Table A15: Peru 2022: Sensitivity of the Discontinuity to Bandwidth

Bandwidth	Schools added	Added any	Risk (simulated)	Unplaced (realized)
$h = 0.050$	+2.456 (2.747)	+0.067 (0.127)	−0.001 (0.016)	+0.000 (0.000)
$h = 0.075$	+1.784 (1.948)	+0.054 (0.090)	+0.006 (0.015)	−0.012 (0.012)
$h = 0.100$	+1.362 (1.430)	+0.070 (0.068)	+0.000 (0.013)	−0.013 (0.011)
$h = 0.150$	+0.861 (0.843)	+0.064 (0.049)	+0.001 (0.010)	+0.002 (0.010)
$h = 0.200$	+0.676 (0.646)	+0.061 (0.043)	+0.005 (0.010)	−0.007 (0.015)
$h = 0.300$	+0.427 (0.418)	+0.078** (0.035)	+0.006 (0.009)	−0.022 (0.015)
$h = 0.400$	+0.413 (0.305)	+0.105*** (0.031)	−0.006 (0.008)	−0.033** (0.015)
$h = 0.500$	+0.405 (0.249)	+0.115*** (0.029)	−0.016** (0.008)	−0.044*** (0.015)
MSE-optimal ($h \approx 0.14$)	+0.795 (0.767)	+0.064 (0.049)	+0.002 (0.014)	−0.001 (0.009)
$h = (0.3, 0.7)$, triangular	+0.371* (0.207)	+0.105*** (0.026)	−0.025*** (0.008)	−0.047*** (0.016)
$h = (0.3, 0.7)$, uniform (Fig. 7)	+0.341** (0.157)	+0.114*** (0.027)	−0.039*** (0.011)	−0.077*** (0.020)

Notes: Local linear estimates at the 0.30 cutoff of the operational risk score, triangular kernel unless noted, on applicants in the list arms (T_2 , T_{3-2022}) whose report-card link was delivered ($N = 939$; 277 below the cutoff and 662 above), the sample of Figure 7. Conventional coefficients with standard errors beneath. The final three rows give the MSE-optimal bandwidth selected separately for each outcome, the fixed asymmetric bandwidth used in the paper’s estimates, and that same bandwidth with the uniform kernel, which is the specification drawn in Figure 7. The realized-placement estimate at $h = 0.050$ is degenerate (0.000, s.e. 0.000) and uninformative. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A16 holds the bandwidth fixed at 0.15 and moves the cutoff. A genuine discontinuity should appear at 0.30 and nowhere else. For the risk outcomes the true cutoff yields nothing (0.001, s.e. 0.010, on simulated risk and 0.002, s.e. 0.010, on realized non-placement), and the six placebo cutoffs yield estimates as large as the true one: point estimates of both signs, none distinguishable from zero at this bandwidth. The risk estimates become significant only once the window is 0.4

or wider in Table A15, where it covers most of the support and the estimator is fitting the slope of risk on risk rather than a jump. The placebos for the behavioral outcomes also move: “added any” jumps by 0.016 at a 0.15 cutoff, significant at the 5% level, and schools added by -0.244 (s.e. 0.127) at 0.20, significant at the 10% level, although nothing was assigned at either cutoff. We therefore do not report a discontinuity estimate for placement risk in 2022; Table 10 marks it as not identified and records the discontinuity in schools added as positive and imprecise.

Table A16: Peru 2022: Placebo Cutoffs at a Fixed Bandwidth

Cutoff	Schools added	Added any	Risk (simulated)	Unplaced (realized)
0.15	-0.002 (0.012)	$+0.016^{**}$ (0.007)	-0.002 (0.013)	-0.027 (0.029)
0.20	-0.244^* (0.127)	-0.009 (0.008)	-0.001 (0.009)	-0.001 (0.001)
0.25	$+0.233$ (0.183)	-0.011 (0.011)	-0.006 (0.014)	-0.003 (0.007)
0.30 (true)	$+0.861$ (0.843)	$+0.064$ (0.049)	$+0.001$ (0.010)	$+0.002$ (0.010)
0.35	-0.409 (0.253)	-0.035 (0.073)	$+0.020$ (0.016)	$+0.032$ (0.049)
0.40	$+0.122$ (0.229)	$+0.044$ (0.078)	-0.025 (0.020)	-0.025 (0.040)
0.45	$+0.230$ (0.277)	$+0.009$ (0.107)	-0.014 (0.019)	$+0.091$ (0.099)

Notes: As in Table A15, with the bandwidth fixed at 0.15 and the cutoff moved. The true policy cutoff is 0.30; every other row is a placebo at which no warning was assigned. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A17 checks what else moves at the threshold among list-arm applicants. Delivery of the report-card link, opening the card, gender and district show no discontinuity at 0.30 at either bandwidth. The grade mix does differ across the cutoff at $h = 0.30$: the first-grade share is lower above it by 0.10 and the pre-pre-kindergarten share higher by 0.07 (robust $p \leq 0.01$), as expected when grade

composition changes with risk and a wide window fits that gradient linearly; at the MSE-optimal bandwidth neither differs from zero, and the initial list length differs only in the narrowest window (0.77, s.e. 0.41, at $h = 0.05$). Adding these covariates to the local linear fit leaves the jump in adding any school unchanged: 0.078 (s.e. 0.030) at $h = 0.30$ and 0.074 (s.e. 0.042) at the MSE-optimal bandwidth among all assigned list-arm applicants, against 0.077 and 0.068 without them (last row of the table).

Table A17: Peru 2022: Covariate Continuity at the Threshold

Outcome	List arms, all assigned		List arms, link delivered	
	$h = 0.30$	MSE-optimal	$h = 0.30$	MSE-optimal
Report-card link delivered	−0.043 (0.060)	−0.020 (0.130)	—	—
		$h = 0.09$		
Opened the report card	−0.129 (0.084)	−0.011 (0.129)	−0.117 (0.090)	−0.037 (0.162)
		$h = 0.13$		$h = 0.11$
Initial list length	+0.164 (0.173)	+0.767** (0.407)	+0.201 (0.194)	+0.831* (0.511)
		$h = 0.05$		$h = 0.05$
First grade	−0.104** (0.073)	−0.185 (0.143)	−0.108** (0.082)	−0.192 (0.170)
		$h = 0.09$		$h = 0.10$
Pre-pre-kindergarten	+0.069*** (0.064)	+0.224 (0.110)	+0.074*** (0.071)	+0.271* (0.131)
		$h = 0.10$		$h = 0.10$
Female	+0.098 (0.083)	+0.154 (0.159)	+0.091 (0.088)	+0.111 (0.162)
		$h = 0.10$		$h = 0.11$
Largest district	−0.006 (0.068)	+0.090 (0.170)	+0.049 (0.072)	+0.206 (0.173)
		$h = 0.07$		$h = 0.06$
Added any school, covariates in the fit	+0.078 (0.030)	+0.074 (0.042)	+0.082 (0.035)	+0.067 (0.048)
		$h = 0.13$		$h = 0.15$

Notes: Local linear estimates (triangular kernel, **rdrobust**) of the jump at the 0.30 cutoff of the operational risk score in predetermined characteristics and in the two delivery margins, among list-arm applicants (T_2 , T_{3-2022}): all assigned ($N = 1,081$) and those whose report-card link was delivered ($N = 939$, where delivery is constant and its row is blank). Columns give the estimate at $h = 0.30$ and at the MSE-optimal bandwidth selected for each outcome (shown beneath). Conventional standard errors in parentheses; stars use the robust bias-corrected p -value. Gender and grade are randomization strata. The last row re-estimates the jump in adding any school with grade, initial list length, gender and district included in the local linear fit (**rdrobust**, **covs**). Source: `codeAnalysis/rd.by_arm_peru2022.py`. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A18: Peru 2022: Density of the Risk Score at the Cutoff

Sample	N below	N above	Bin $[0.25, 0.30)$	Bin $[0.30, 0.35)$	T	p
All applicants	473	1140	71	96	-0.26	0.79
List arms (T_2, T_3)	318	763	52	70	0.98	0.33
No list (T_1)	155	377	19	26	-0.82	0.41

Notes: Local polynomial density test of [Cattaneo et al. \(2020\)](#) (`rddensity`, jackknife standard errors, data-driven bandwidths on each side) for a discontinuity in the density of the operational risk score at 0.30, for the samples of Table 3. N below and above count applicants with a positive score on each side of the cutoff; the bin columns count applicants within 0.05 of it. T is the robust bias-corrected statistic and p its two-sided p -value. Source: `codeAnalysis/rd_density_peru2022.py`.

H Survey Knowledge of Listed Schools

The list-knowledge outcome in Panel C of Table A8 is the share of the surveyed listed schools that the respondent reports knowing well or by name. It combines two survey questions: a grid on up to five listed schools that were not in the family’s application, and the question on the first five schools in the family’s own application, for any listed school that appears there. Because the lists excluded the schools in the initial application, the second component contains only listed schools that the family added afterwards, and T_1 openers report knowing more than 90% of the schools they applied to. Where the list changed which schools families added, the outcome therefore rises partly by construction. Unless stated otherwise, the estimates below are $T_2 - T_1$ differences among respondents who opened the report card, with strata controls and HC1 standard errors.

In Peru 2021, the T_2 effect is 0.113 (s.e. 0.055) on the full outcome, against 0.143 without strata controls in Table A8, and 0.056 (s.e. 0.056) on the grid items alone. The grid-only estimate is a lower bound, because the grid omits any listed school that a family learned of and then added. In Ecuador, where the list did not detectably change which schools families added, the two versions give the same estimate, -0.070 (s.e. 0.032 on the full outcome and 0.033 on the grid items), against -0.074 without strata controls in Table A8. On the grid items, the Ecuador–Peru difference is -0.126 (s.e. 0.065), smaller than on the full outcome but not eliminated.

The Ecuadorian decline is not confined to listed schools. Relative to T_1 , the share of listed schools that T_2 respondents know only by name falls by 0.063 (s.e. 0.030) and the share they do not know rises by 0.070 (s.e. 0.033). On the family’s own applied schools, the share known well falls by 0.093 (s.e. 0.047) and the share known at all by 0.055 (s.e. 0.028). Within respondents, the decline on listed schools exceeds the decline on own schools by an insignificant 0.027 (s.e. 0.040), and knowledge of nearby private schools, which no list named, falls by an insignificant 0.032 (s.e. 0.040). Among all respondents, including those who did not open the card, the decline on the listed grid items is -0.045 (s.e. 0.022), while answers on own schools (-0.027 , s.e. 0.018) and private schools (0.001, s.e. 0.026) move less. In T_{3-2021} , whose card also carried the list, own-school

knowledge does not fall (0.026, s.e. 0.020), and knowledge of listed schools falls relative to own schools by 0.079 (s.e. 0.035), although T_{3-2021} respondents also report knowing fewer nearby private schools, which no list named (-0.069 , s.e. 0.038). T_2 and T_1 openers responded to the survey at similar rates (difference -0.046 , s.e. 0.043), and respondents in the two arms are balanced on baseline risk, warning display, initial application length, local school density and outreach variables (all $p \geq 0.67$), although fewer T_2 respondents answered after the first survey day (3% against 11%). The T_2 pattern is consistent with respondents answering knowledge questions more conservatively, but the within-respondent contrast is too imprecise to rule out a decline specific to the listed schools, and the T_{3-2021} contrast is mixed. The survey evidence therefore does not establish that the list reduced awareness in Ecuador, nor does it rule it out.

I Data Sources and Documentation

We use two primary data sources covering Ecuador 2021 and Peru 2021–2022: (i) administrative records from the centralized assignment platforms and (ii) an online applicant survey. The administrative data cover the universe of applicants who submit a rank-order list (ROL), the set of schools/programs available on the platform, each applicant’s submitted ROL (and, where available, the history of ROL edits), and outcomes from the deferred acceptance assignment as well as post-assignment enrollment outcomes recorded by the Ministries of Education. The survey (implemented on Qualtrics and distributed via WhatsApp) elicits beliefs about assignment probabilities, knowledge of local on- and off-platform options, and perceptions of the application process; it is personalized using applicants’ administrative records (e.g., applicant name and schools in the submitted ROL) and merged back to administrative data using the applicant identifier.

Data availability and confidentiality. The administrative and survey microdata contain sensitive personal information and are restricted by the data-use terms described in the Data Availability statement, so neither the raw records nor de-identified extracts are part of the replication package, which contains all code and documentation. The Data Availability statement also describes how to request the microdata from each ministry.